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ORIGINAL ARTICLES.

RETARDATION OF THE PULSE IN MITRAL INSUFFICIENCY.

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Read in the Section of Practice of Medicine and Materia Medica of American Medical Association, May, 1884.

At the meeting of the Association last year, I read before this section a paper on Diminution of the Retardation of the Pulse in Aortic Insufficiency, in which were considered the reality, mechanism and diagnostic value of the phenomenon. In the present paper I wish to present the subject of augmentation of the retardation of the pulse in mitral insufficiency.

This subject in its inception and literature seems to be peculiarly my own. For although none would question the importance of a new diagnostic sign of a lesion so common and grave as mitral insufficiency, and whose diagnosis by means of the old signs is not always infallible; and although I presented to the profession some years ago demonstrations of this phenomenon of mitral regurgitation which to me were conclusive, the announcement has attracted but little attention; my observations have neither been confirmed nor disproved by others, and two writers only have noticed them. Dr. A. B. Isham, in presenting the value of the graphic method from the point of view of examinations for life insurance, accepts abnormal delay of the pulse as a valuable sign of mitral regurgitation.[1] And M. Pélix, in his inaugural thesis on retardation of the pulse, quotes largely and approvingly from my published reports[2]. These papers and my own contain all that has been contributed upon the subject.

Convinced myself of the truth and intrinsic value of the observation, and desiring to see the fact accepted and utilized, I have prepared this article, hoping that a new presentation of the subject may lead to new interest in the phenomenon, and stimulate personal investigation of the time-relation between the heart and pulse in real or suspected mitral insufficiency.

[1] *American Journal of the Medical Sciences*, July, 1882, p. 119.
[2] Paris, 1882.

Evidences of the reality of the phenomenon are drawn, 1, from a consideration of the mechanism of mitral insufficiency; 2, from experiments on the schema; 3, from experimental observations on man.

Examining the question first from the point of view of the mechanism of mitral insufficiency we shall find a convincing *a priori* argument in favor of abnormal delay of the pulse as a direct effect of this lesion. Under normal conditions with mitral valve intact, the heart is a perfect organic pump, the left ventricle alternately and duly receiving and sending out blood. When the ventricle contracts, the pressure within becomes positive in relation to the arterial and auricular, which insures the prompt closure of the auriculo-ventricular valve and immediately thereupon the opening of the aortic valves and escape of blood into the aorta.

When the ventricle relaxes, the pressure becomes negative with respect to the arterial and auricular, and the blood flows freely in from the auricle, but is prevented doing so from the artery by closure of the semilunar valves. Auricular systole raises somewhat the diastolic intra-ventricular pressure, but the moment of beginning systole finds a considerable excess of pressure on the side of the artery. The time required to overcome this excess after the beginning of ventricular systole measures the duration of the ventriculo-aortic or presphygmic interval, which under the normal conditions will give a normal value.

Thus all goes smoothly on; the rhythmic succession of contraction and relaxation, of valvular opening and closure, of inflow and outflow of blood, and all the events of a cardiac cycle in harmony with each other in time and status and movement.

But when the mitral valve is permanently open the harmony is no longer preserved. The ventricle fills in diastole even more promptly and completely than in normal conditions, in consequence of falling into it of blood from the surcharged auricle; but in systole the blood is first sent backwards through the open valve into the relaxed auricle. This process fills the auricle and pulmonary veins and dams back the blood towards the lungs. The retrograde current, however, is soon arrested by meeting the direct current from the right ventricle, whose systole is synchronous with the left, and whose tricuspid valve is intact. Thus the pressure within the left ventricle and auricle (now one cavity) finally becomes positive with respect to the aortic pressure, and for the remaining part of the systole blood escapes into the aorta. From this it follows that the time and work of each

systole is consumed in first driving the blood backwards, and only a part of the systole is effective in driving the blood into the general arterial system. The effect of this condition of things upon the arterial pulse is to postpone the time of its appearance and lessen the volume of its wave; the cardiac systolic portion is shortened relatively to the length of the cardiac systole. If the heart maintain its vigor the pulse may still feel sharp to the fingers and its sphygmogram give a steep ascent and pointed summit; yet it quickly vanishes to the fingers, and, as shown in the trace, it lacks duration, as compared with the duration of the systole which has sent it forth. If the heart passes into irregular action there will be systoles without arterial pulsation, and pulsations presenting a great variety in form and in time. The many and grave ulterior pathological sequences I will not follow, as the immediate mechanism is all that concerns the present study.

If the above sketch of the mechanism of mitral insufficiency be truly drawn, the conclusion inevitably follows that abnormal delay of the pulse is an essential phenomenon of the lesion.

EXPERIMENTS ON THE SCHEMA IN ILLUSTRATION AND EVIDENCE.

Passing next to the results of experiment, I will first offer those obtained with the schema. It is well known that the mechanical action of the circulation can be well imitated by means of a good working schema. With such a device connected with the transmission tubes of an apparatus for simultaneous inscriptions, traces were obtained at the same time from the interior of the ventricle and interior of the aorta. The intra-ventricular pressure in diastole was maintained at 4 inches, and the intra-aortic at 20 inches (water manometer). The ventricle was compressed and decompressed in a uniform manner for all the experiments. The conditions were varied only with respect to the mitral valve. The instant of ventricular impulsion and that of the aortic wave were signaled by the ascents of the traces, and the time-difference between these ascents was easily computed by the accompanying chronogram. Experiments were first made with the mitral valve intact, representing normal conditions. Fig. 1 is a fair example of the results. Delay of the aortic wave, $^1/_{16}$ second.

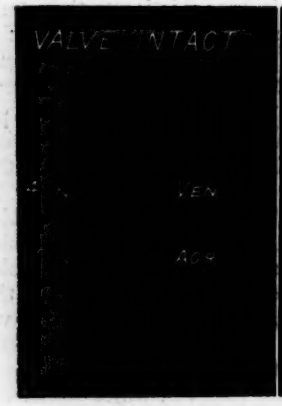

Fig. 1.

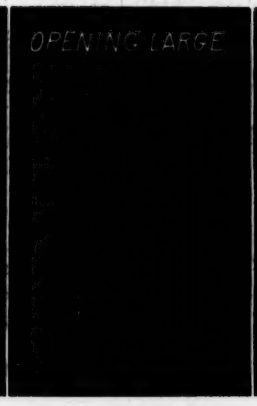

Fig. 2.

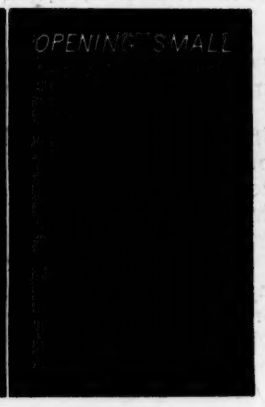

Fig. 3.

Another series of experiments was made with a large mitral opening, of which Fig. 2 shows an average result. Aortic delay, $^2/_9$ second—more than three times as long as the result with intact valve.

A third series of experiments was made with a comparatively small mitral opening, with result shown in Fig. 3. Aortic delay, $^1/_9$ second, which is seen to be intermediate between the other two.

These results well illustrate the retarding effect of mitral insufficiency on the time of the pulse wave, and the additional significant fact that the retardation is in proportion to the size of the mitral opening. Indeed, in view of the careful manner in which the apparatus was prepared and the experiments conducted, together with the analogy in all essential points for our purpose between the schema and heart, these results may be admitted, also, as veritable demonstrations of the phenomenon and phases under consideration.

EXPERIMENTS ON MAN.

The subject remains to be further elucidated by the results of experimental observations on man.

In order to appreciate chronometric discrepancies between the heart and pulse, it is essential that the normal cardio-arterial time-relations should be well established. This has been done. The interval between the beginning of ventricular systole and the carotid pulse has been found, after a great many observations and measurements, to average about $^1/_{12}$ second with pulse-rate at 72. The normal range of variation with this pulse-rate may be placed between $^1/_{11}$ and $^1/_{13}$ second. It has been determined that the interval bears a relation to the pulse-rate, so that a frequency of 120 would give an interval perhaps not more than half as long as a frequency of 60 per minute. Also investigations have shown that the causes of the variations with the pulse-rate are changes in the mode of ventricular systole and variations of the arterial blood-pressure relative to the ventricular. In frequent pulsations, the systoles being quicker and the arterial pressure relatively lower, the cardio-carotid interval would be correspondingly shortened. The rule is this, the cardio-carotid interval for a pulse frequency between 60 and

120 varies inversely with the pulse-rate; so in estimating what would be a normal interval in a given case the rate of the pulse must always be taken into account.

The interval between the heart and more distant arterial points, as the radial, femoral, or posterior tibial is complicated by the element of transmission time of the pulse wave, which is also subject to variations; but the transmission interval between the aortic orifice and carotid point is so short compared with the whole cardio-carotid interval, as measured, that its variations are not appreciably apparent. Hence the importance of choosing the carotid or subclavian for observing the cardio-arterial time-interval.

The cardio-carotid interval, then, is practically the presphygmic or time elapsing between the beginning of ventricular contraction and opening of the aortic valves, and variations of delay of the carotid pulse are virtually variations in the length of the presphygmic interval.

With these data concerning the physiological duration and variations of the cardio-carotid interval we are prepared to signal and appreciate the changes effected by conditions of disease.

Our immediate study concerns delay of the pulse in mitral insufficiency. The only clinical data we possess relating to this point have been furnished by my own researches.

My first observation was made in July, 1877, on a boy, aged 9 years, who suffered from mitral insufficiency, and subsequently dying, a post-mortem revealed in his case a pure mitral regurgitant lesion without any other valvular change. His pulse at the time of observation was 100 per minute. Simultaneous traces of his heart and carotid gave an interval of $^{2}/_{17}$ second, when about $^{1}/_{17}$ second would have been his normal interval with his pulse-rate. Also simultaneous traces of his heart and radial gave an interval of a little less than $^{1}/_{5}$ second; and the same of the heart and posterior tibial a little more than $^{1}/_{4}$ second; both the latter also abnormally long. These results are shown in figure 4.

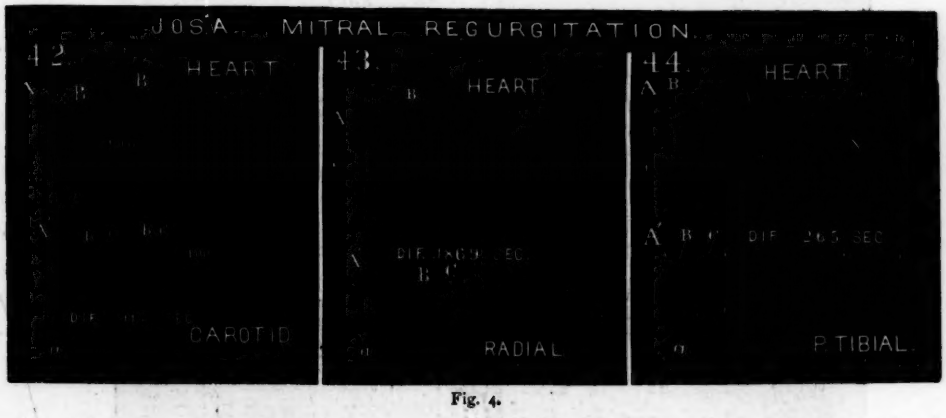

Fig. 4.

Soon after this case I traced another; a young man aged 20 years who presented all the diagnostic signs of mitral insufficiency. Simultaneous traces were had of his heart and radial which gave an interval of $^{2}/_{9}$ second with pulse-rate at 80. If we deduct from this interval $^{1}/_{7}$ second, as an average carotid-radial transmission time, we get a value between $^{1}/_{6}$ and $^{1}/_{7}$ second as the approximate cardio-carotid interval in this case. The normal interval with pulse at 80 would be near $^{1}/_{13}$ second. Traces shown in figure 5.

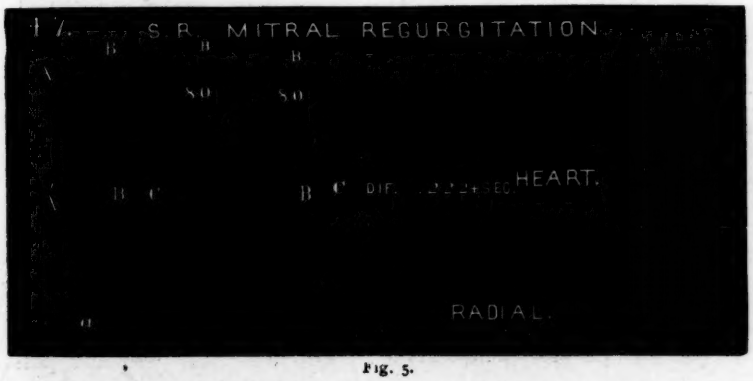

Fig. 5.

Observations from these two cases formed the basis of a paper which was published in the Cincinnati *Lancet and Clinic*, March 22, 1879. Since these cases I have continued my observations with results which only multiply the demonstrations of the fact that the pulse is abnormally delayed in mitral regurgitation. But as demonstrations are more conclusive than mere statements of results, the simul-

taneous cardio-arterial traces (with accompanying chronogram in fifths of seconds), from a number of patients presenting diagnostic signs of mitral regurgitant lesion are here produced.

The cardio-arterial intervals in all these cases of mitral insufficiency are abnormally great; greater than those obtained from persons in health, or from those affected with any process of disease which does not involve pulse-retarding lesion of the heart or aorta. In the absence of such lesion, no series of cases with corresponding pulse frequencies can be produced which will show such an amount of pulse-retardation.

Figures 6 and 7 are from Jennie M., aged 11 years. She had suffered with chorea and recurring rheumatism, and her heart had become seriously affected. It was enlarged, and emitted a loud murmur, which was distinguished as mitral regurgitant. Fig. 6 was taken during an acute attack, in the presence of fever, pulse-rate 140. Fig 7 was taken after subsidence of acute symptoms, pulse-rate 114.

Fig. 6.

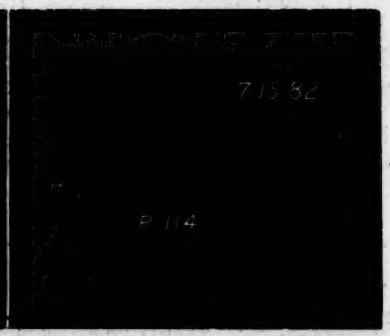

Fig. 7.

Some months subsequently the girl experienced a new attack, with grave cardiac implication, under which she died. The post-mortem revealed pericarditis old and new, with adhesions and liquid effusion; general enlargement of the heart, especially hypertrophy of the right side; notable insufficiency of the mitral valve, the result of inflammatory thickening and contraction; the other valves normal.

Fig. 8 is from Mary M., next younger sister of Jennie, also aged 11 years at the time of observation. She became affected with chorea and sub-acute rheumatic symptoms, and when first seen her heart was notably enlarged, and there was present a distinct apex systolic murmur. She had slight fever, pulse-rate 114. Carotid delay shows $^{1}/_{10}$ second against $^{1}/_{19}$ which would be about her normal interval.

Fig. 8.

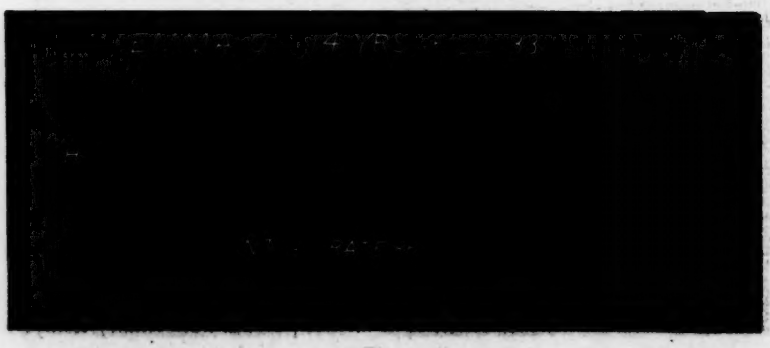

Fig. 9.

Fig. 9 is from Emma O., who presented an apex systolic murmur, which supervened during an attack of scarlet fever, and persisted after recovery from that disease. At the time of observation, June 22, 1883, the murmur and pulse delay were the only indications of cardiac lesion. She remains well to the present time.

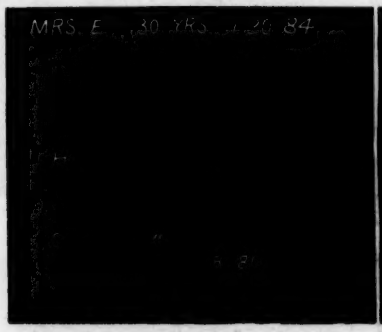

Fig. 10.

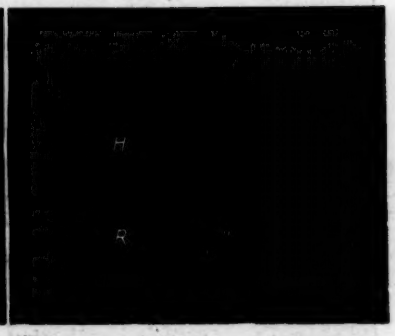

Fig. 11.

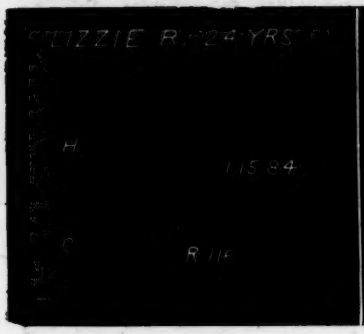

Fig. 12.

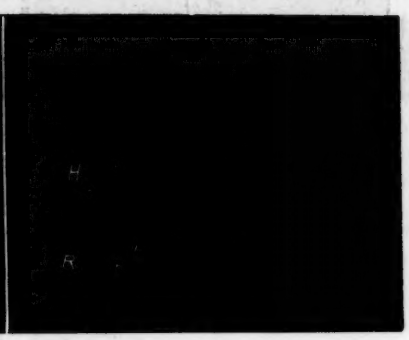

Fig. 13.

Fig. 14.

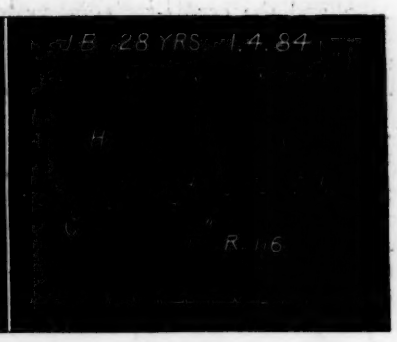

Fig. 15.

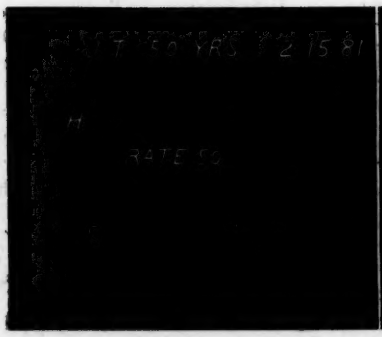

Fig. 16.

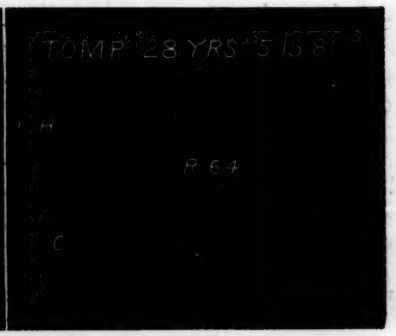

Fig. 17.

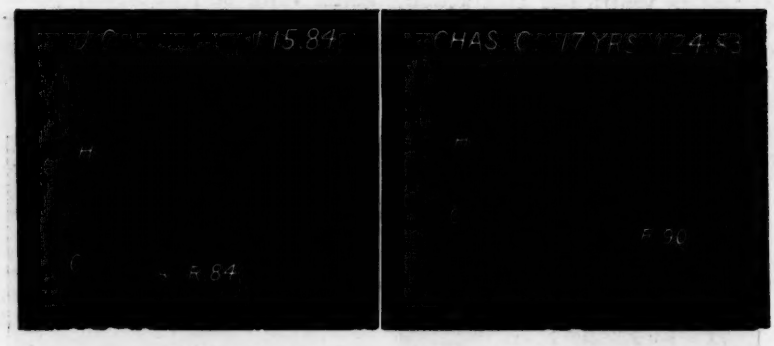

Fig. 18.

Fig. 19.

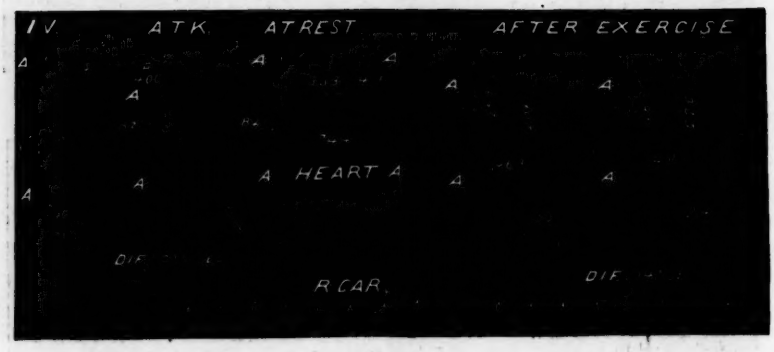

Fig. 20.

Figures 10 and 11 are from Mrs. E., aged 30 years. She was taken with præcordial soreness and a dry, hacking cough, and, when seen a few days afterwards, betrayed a marked apex systolic murmur. Tracings show the cardiac-carotid interval to be $^1/_9$ second with pulse-rate at 80, and the cardio-radial interval to be $^1/_6$ second.

Figures 12 and 13 are from Lizzie R., aged 24 years, hospital case, presenting apex systolic murmur. Delay as shown.

Fig. 14 is from Geo. B., aged 27 years, presenting apex systolic murmur. Delay as shown.

Fig. 15 is from J. B., aged 28 years, hospital case, with diagnosis of mitral regurgitation and cardiac hypertrophy. At the time the traces were taken he was under a febrile paroxysm. Indications and delay as shown.

Fig. 16 is from J. T., aged 50 years, a hospital patient, with cardiac enlargement and apex systolic murmur. Pulse delay and pulse-rate as shown.

Fig. 17 is from Sam. T., aged 28 years, who presented the signs of mitral insufficiency.

Fig. 18 is from J. C., a hospital case, with well-marked signs and symptoms of cardiac hypertrophy and mitral regurgitation.

Fig. 19 is from Chas C., aged 18 years, with indications of mitral insufficiency.

Fig. 20 is from a man in health, aged 54 years. The first traces were taken when the circulation was quiet, pulse 82; the second traces when the circulation was accelerated as consequence of exercise, pulse 130. In this figure the pulse delay represents the normal amount. It measures, in fractions, a little less than $^1/_{13}$ second, with pulse at 82, and a little less than $^1/_{21}$ second with pulse at 130.

Having thus shown that the mechanism of mitral insufficiency includes increased delay of the pulse as one of its essential phenomena, and demonstrated the constant relationship between the lesion and phenomenon, both on the schema and man, I submit the proposition as proven, and the fact as true beyond a doubt.

DIAGNOSTIC VALUE.

In undue retardation of the arterial pulse on the systole of the ventricle we have a special and highly diagnostic sign in mitral insufficiency. In this lesion, when pure and effective, the sign is always present, and fails to manifest only when the lesion is so slight as to cause no functional disturbance, or when complicated with large insufficiency of the aortic valves. In the condition of harmless regurgitation through the mitral valve the retardation of the pulse remains normal, because the conditions are virtually normal; in the complication stated the retardation is abnormally diminished, for the reason that the aorta ventricle and auricle are as one cavity, with blood-pressure in equilibrium at the end of ventricular diastole, and the beginning of systole starts a current forwards into the artery as well as backwards into the auricle. Thus in mitral, complicated with free aortic insufficiency, instead of abnormal delay, there is abnormal precipitation of the pulse. These are the only exceptions to the rule, that the sign is present when the lesion is present.

What other conditions besides the prime one considered are capable of determining undue retardation

of the pulse? Slowness of ventricular contraction, and relatively high arterial pressure are known causes of prolongation of the presphygmic interval, but these causes are considered to produce only the normal extreme of prolongation.

1. There are good reasons to believe that mitral stenosis independent of insufficiency is capable of causing a delay which may exceed the normal limits. The mechanism, and experiments on the schema favor this view; besides, not long since I traced a case presenting the presystolic murmur and other clinical features of pure mitral stenosis and found the cardio-carotid interval unduly long. If this fact should become established, (of which I have no doubt) it will be found that the delay, unless in extreme narrowing, will not be as great as in mitral regurgitation, and that the sign will not manifest at all in moderate or even considerable contraction, for it would require a very small opening to prevent the ventricle from duly filling during diastole, which would be requisite to insure the production of the sign. Even admitting the fact of the relationship, the sign can have nothing like the diagnostic significance in mitral constriction that it has in mitral regurgitation.

(2.) Rarely, increased delay may be produced by a "locking" of pathologically changed aortic valves. This condition is uncommon, but may obtain when the valves are stiff and thickened, and, notably, when loaded with calcareous masses. They close with a species of locking and are forced open only after a prolonged effort of the ventricle.

(3.) It has been well established that an intervening thin-walled distensible aneurism of the ascending aorta may cause increased delay of the distal pulse, but that if the aneurism have resisting walls, even though it be large, there is no increased delay. Evidently, if the pulse could be traced on the proximal side of the pouch, which is impracticable in man, the delay would not obtain.

The conditions have now all been named in which there is a possibility of undue delay of the pulse being produced by other causes than mitral insufficiency. With the latter all are serious organic lesions of which abnormal delay of the pulse may be a common product.

The sign then, although always present in pure, effectual mitral insufficiency, is not of itself pathognomonic of this lesion, since it may be present, also, in other organic conditions. Here the stethoscope comes beautifully to the aid of the graphic instrument, and in the union of the apex systolic murmur of the one and the pulse-delay of the other, our diagnosis is perfected.

The graphic sign compared with apex systolic murmur.—Systolic murmur located at the left apex, is the physical sign above all upon which turns the diagnosis of mitral insufficiency. Yet all know there may be free regurgitation without murmur, and insignificant harmless regurgitation with loud murmur, and that no direct relation subsists between the amount of regurgitation and the intensity of this auscultatory sign. Also is it well-known that apex systolic murmur may be distinctly present and the mitral valve be functionally perfect. These points do not compare altogether favorably with the point we have stated as true concerning the graphic signs The comparison is well shown by apposition.

Abnormal pulse delay.	*Apex systolic murmur.*
Always present in pure, harmful, mitral regurgitation.	Sometimes absent in very free regurgitation.
Always absent in harmless regurgitation.	Usually present and often very loud in harmless regurgitation.
The amount of delay directly proportional to the amount of regurgitation.	No certainty that the intensity of the murmur is directly proportional to the amount of the regurgitation, but often it is inversely so.
Present in other conditions besides mitral regurgitation, but these are organic and serious.	Present in other conditions besides mitral regurgitation, but these, if organic, are harmless.

Although the graphic sign has greatly the advantage in the comparison, its value and utility are most manifest when it is obtained and considered in connection with the auscultatory. Alone it is not competent to declare the presence of the lesion, but in association with systolic murmur at the left apex it speaks with positive import. Neither sign singly is conclusive of the lesion, but the two combined could be produced by no other. One locates the lesion, the other determines the extent of its damage.

An important point is that the diagnosis as made by the aid of the graphic sign is certain and immediate at any stage of the case, while the diagnosis as made by all other indications, exclusive of this, is not certain until confirmed by the development of pathological sequences. When, as often happens, other signs raise the question of the existence or not of harmful regurgitation, the graphic comes in with its prompt and positive decision.

RÉSUMÉ OF CONCLUSIONS.

1. Abnormal retardation of the arterial pulse, notably the carotid, on the systole of the ventricle is a real phenomenon of mitral insufficiency.

2. It is present in all cases of pure, harmful, mitral insufficiency, and is absent only in insignificant, harmless, regurgitation, or in regurgitation complicated with aortic insufficiency.

3. It measures by the amount of retardation the amount of the regurgitation.

4. It may be present in two other conditions, probably a third, all organic; hence this sign, notwithstanding its positive value, is not of itself pathognomonic.

5. In conjunction with apex systolic murmur its presence is conclusive of mitral insufficiency.

6. Compared with apex systolic murmur, this sign is more positive and appreciative, and distinguishes, which the latter does not, between harmful regurgitation and harmless conditions.

7. By the aid of this sign a positive diagnosis ma be made at once in any stage of the case, and without waiting for the development of sequences and symptoms.

AMPUTATION AT THE HIP JOINT, WITH A REVIEW OF THE VARIOUS METHODS OF CONTROLLING HÆMORRHAGE.

BY C. A. WHEATON, M.D., ST. PAUL, MINN.

Read in the Section of Surgery and Anatomy of the American Medical Association, May, 1884.

MR. CHAIRMAN AND GENTLEMEN OF THE SURGICAL SECTION: You may perhaps think I display more of temerity than good judgment, in offering for your consideration an essay on this time-honored and much discussed subject, but my recent experience with two cases of Coxo-femoral amputation where the hæmorrhage was effectually controlled after the simple method recently described by Mr. Loyd in the *Lancet* of May 26, 1883, has led me to review the literature at my command of the operation since its first performance by Lacroix, of Orleans, in the year 1748. From the time of the first formal disarticulation at this joint to the middle of the present century, the hæmorrhage was controlled or sought to be, by digital compression against the pubic ramus at Poupart's ligament; by securing the crural vessels at that point, or below in Scarpa's triangle, or by the fingers of an assistant following the knife of the operator in fashioning the anterior flaps. We are told by Dr. Otis, that in 1845, "Mr. Cox advised that the arterial compressor, invented by Signoroni, of Padua, should be employed in amputations at the hip joint to control the arterial circulation at the groin without impeding the return of blood by the veins." In the year 1860 Prof. Joseph Pancoast introduced in Philadelphia the compressor for the abdominal aorta; a little later a modification of the same instrument was used in England by Professors Lister, Syme, and others, and by the French and German surgeons on the continent. Since that time innumerable devices have sprung up, having the same end in view, viz.: that of controlling the circulation through the terminal branches of the common iliac. Since its introduction the aortic compressor has been considered by many an almost indispensable adjunct to the operation. Nearly every modern surgical textbook speaks favorably of it, but a few writers, however, still cling to the older method of securing the femoral vessels by a preliminary operation. What are the objections to digital compression of the femoral as it emerges from beneath Poupart's ligament? Davy speaks of it thus, in comparing it with his ingenious device, the rectal lever: "I can only state that after a surgeon has proved, practically, how very easy it is to prolong indefinitely compression by the lever, he will not care to submit to the fatigue, uncertainty, and anxiety of digital pressure." The degree of pressure must fluctuate with the movements of the patient; in fat subjects it is difficult, extremely fatiguing, and must be at times uncertain, and withal, even though accurately and efficiently done, only one of the two avenues through which impending calamity is to be anticipated, is controlled, for I believe all surgeons stand as much in dread of the posterior as the anterior vessels. Digital compression of the aorta may be resorted to, but except in the case of young or much emaciated subjects entails the same objections as attach to femoral compression, only greater in degree, and has the additional objection of interrupting the circulation to both limbs, and interfering in some degree with abdominal respiration.

The objections to the preliminary ligation of the femoral vessels are, first, the additional time required; second, an additional wound, and thirdly, as Dr. Ashurst suggests, "that the extensive separation of the artery from the surrounding tissues, which is unavoidable in this preliminary ligation, must expose the patient to greater risk of secondary bleeding than when the vessel is simply picked up by forceps or tenaculum after division as in other amputations." With reference to the abdominal tourniquet, to quote again from Dr. Ashurst's surgery, "although the benefits derived from the use of the aortic compressor in this operation are unquestionable, yet the pressure necessarily exercised upon the important nervous structures contained in the abdominal cavity must be at least undesirable, if not positively injurious." There is the possibility of a deviation of the artery from the mesial line sufficiently to make its control by pressure a matter of some difficulty. The interference with abdominal respiration in weakly subjects under anæsthesia may induce much additional embarrassment. The possibility of making harmful pressure; the interference with the circulation to the sound as well as the unhealthy limb; its absolute inapplicability to those cases requiring prolonged control of the circulation, such as sequestrotomy, excision, or the removal of extensive growths from the upper femoral region—all these objections certainly militate much against its general usefulness. Until quite recently Davy's lever was, perhaps, the most popular and efficient instrument which had been introduced to the profession for this purpose. It was easy of application; not likely to become displaced when once in position; did not interfere with the respiratory movements; the pressure was confined to the common iliac of the affected side, and was efficient without embarrassing in any way the operator. It was supposed to be entirely harmless, but unfortunately, not long since, a death occurred, caused, it is said, by peritonitis, which had resulted from the pressure of the instrument, this accident occurring when the instrument was in the hands of its inventor. While it serves its purpose so admirably in many respects, yet I think its having been the cause of a fatal issue in this individual case will be sufficient to circumscribe its general use. The hand introduced into the rectum has been used as a substitute for the lever, but in a case reported by Dr. Thorndike at the Boston City Hospital, it was found impossible to adopt this method, as the arm of the assistant was in the way of the operator.

The method of Brandis, spoken of by Esmarch in his "Surgeon's Hand-book," is but a modification of the principle of the abdominal tourniquet, and

has all of its objections without any of its advantages. In the *American Journal of Medical Sciences*, April, 1882, is given a description of a method of amputation at the hip joint by Professor Freudelenburg, "the principle of the method being the compression of the entire tissues of the flaps before the division of the large vessels, indeed, before the flaps are cut," by means of a steel skewer which is made to transfix the thigh in front of the femur, the tissues in front of the rod being compressed by a rubber tube wound in figure of eight fashion from end to end of the rod. The flap is then cut with a Lisfranc's knife, from within outward, as in the usual manner by transfixion. All vessels in this flap are now secured, and the skewer removed. The next step in this operation is the opening of the capsule and disarticulation of the femur, after which the rod is again passed through the limb behind the joint, the compressing band applied, and the posterior flap cut and cared for like the anterior. The objections and disadvantages of such a complicated and protracted operation are sufficiently obvious, and require no further consideration. Dr. Lewis Marshall, in the January number of the *American Journal of Medical Sciences*, reports four cases of amputation at the hip joint, after the manner of Mr. Furneaux Jordan. His description of it briefly is as follows: "If incision has been previously practiced, the existing incision is prolonged to about the middle of the thigh, the femur enucleated, the soft parts cut through with a circular sweep of the knife, and the femoral artery tied." Mr. Jordan, in his single case, controlled the bleeding by a tourniquet on the external iliac. In commenting upon this case, Mr. Jordan says that, "as compared with the ordinary operation of two flaps, the wound was less severe; the cut surfaces were less extensive, and in a manner further removed from the trunk; it was followed by less shock, less hæmorrhage, less opportunity for septic infection. The vessels cut were more easily dealt with. The muscles may be cut on the same level as the skin; the bone being absent, they retract so strongly that the skin readily covers them; its vitality is less endangered, and a great cellular plane is not opened. The bulk of the soft parts of the thigh, especially near the pelvis, lie at the inner side of the femur. Why put a knife through these parts? The surgeon may, if he choose, make the circular sweep before the shaft of the bone is turned out, if precautions against hæmorrhage have been very complete." It will be seen that this method described as Mr. Jordan's is a modification of the old circular amputation, as originally performed by Abernethy and subsequently by Lacanchie, was made with the additional "vertical incision over the trochanter major to facilitate disarticulation," Mr. Jordan dividing the skin, soft parts and bone at the same level, instead of cutting through the skin and retracting it before cutting through the muscles, and by dividing all the tissues at a point more remote from the body. Mr. Loyd's plan for controlling hæmorrhage in amputations at the hip joint was first put in execution at the Queen's Hospital, Birmingham, England, June 17, 1883. He thus describes it: After having emptied the limb of its residual blood, by elevation and gentle friction toward the trunk, "a strip of black India rubber bandage, about two yards long, is to be doubled and passed between the thighs, its centre lying between the tuber ischii of the side to be operated on and the anus.

A common calico thigh roller must next be laid lengthways over the external iliac artery. The ends of the rubber are now to be firmly and steadily drawn in a direction upward and outward, one in front and one behind, to a point above the centre of the iliac crest of the same side. They must he pulled tight enough to check pulsation in the femoral artery. The front part of the band passing across the compress occludes the external iliac, and runs parallel to and above Poupart's ligament. The back half of the band runs across the great sacro sciatic notch, and by compressing the vessels through it, prevents bleeding from the branches of the internal iliac artery. The ends of the bandage thus tightened, must be held by the hand of an assistant placed just above the centre of the iliac crest, the back of the hand being against the surface of the patient's body. It is a good plan to pass the elastic over a slip of wood held in the palm of the hand, so as to diminish the pain attending the prolonged pressure of the rubber bandage. In this way an elastic tourniquet is made to encircle one of the innominate bones; checking the whole blood supply to the lower extremity. When the band is once properly adjusted the assisttant has only to take care that it does not slip away from the compress or over the tuber ischii. The former is prevented by securing pad and band together by a stout safety pin, and the latter by keeping the securing hand well above the iliac crest, or even more safely by looping a tape beneath the elastic near the tuber ischii, passing behind it under the sacrum and having it held in that position. The ligature being altogether above the limb is out of the way of the surgeon, in any operation at or about the hip joint. The great trochanter is fully exposed, the hip being free upward as far as the iliac crest, and inward to the perinæum. The plan is equally applicable in amputation by transfixion, or in excision of the joint." The advantages accredited this method by Mr. Loyd, are as follows: "A minimum disturbance of the general circulation. There is no interference with abdominal respiration. There is no danger of injuring the abdominal contents. The simplicity and accuracy of its application; no previous experience being necessary to compress the vessels, there is no possibility of going wrong. The security with which the vessels are controlled, regardless of the movements of the patient or manipulations of the operator." It is applicable to all cases, and requires no special apparatus. In the light of experience furnished me by the two cases of my own, I am satisfied that this method of Mr. Loyd's is all that is claimed for it; it is simplicity itself, and as efficient as it is simple. With the Esmarch's bandage, and the roller applied as he directs, hæmorrhage is no more to be dreaded than in an amputation in any other part of the limb.

With reference to the form of amputation, too little

prominence by surgical writers, I think, has heretofore been given this modification of the circular amputation suggested (as Dr. Otis tells us in the renowned circular No. vii. of the war department) by Lacanchie in 1350. Besides affording easy access to all the vessels requiring ligation, it has the additional advantage (as pointed out by Surgeon Boutecou, U. S. A.) of permitting the examination of the femur as high as the trochanters before disarticulation, and as before quoted from Furneaux Jordan, the wound is less severe; is in a measure more remote from the trunk; the great bulk of muscles lying on the inner side of the femur are not disturbed, and from this non-interference with this mass of muscles less liability to septic infection follows; the vitality of the flaps is less endangered; the wound is as favorable for drainage as any other, and its performance even by a novice is a matter of comparative ease. In both of my cases a circular division of the limb was made, cutting through skin, muscles and bone on the same plane. The knife was applied about an inch and a half or two inches below the gluteal fold. Immediately on dividing the bone the vessels were secured. A vertical incision was next made beginning above the great trochanter and carried down the outer aspect of the thigh to intersect the circular incision. The remainder of the femur was removed as nearly as possible subperiosteally, the knife being used only in separating the muscular attachments to the trochanter and the division of the capsule. Notwithstanding the muscles were cut so near their origin, the skin afforded ample covering. There was absolutely no active hæmorrhage in either case, except from one or two small articular vessels.

The general adoption of the method of Mr. Loyd will, I believe, rob this amputation of one of its greatest dangers, and will confer an inestimable boon upon the unfortunates who are to suffer exarticulation at this joint, as well as contributing greatly to the comfort of the surgeons who are called upon to make it.

Accompanying this paper are three sketches. No. one (1) shows the method of removing the limb, Nos. two (2) and three (3) the application of the tourniquet.

EFFECTS OF MURIATE OF COCAINE ON THE ORBICULARIS PALPEBRARUM.

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In the report of the following cases it will be my endeavor to simply state the facts as briefly as possible, leaving the profession to judge as to the efficacy of this alkaloid in eye surgery.

In the first case a two per cent. solution was used, but in the nine following ones it was increased to four per cent. No question could be entertained as to its purity, as it came from Merck's, and its peculiar action on the orbicularis corroborated this belief.

CASE I. Machinist, æt. 34. F. B. (Emery) deeply imbedded in the right cornea, of three days' standing. The solution was dropped into the eye every three minutes for fifteen minutes when the body was dug out with a spud. He stated that the operation did not hurt much.

CASE II. Very severe trichiasis, in which Arlt's operation for transplantation had been previously performed, and in which the tarsal margins were very much notched, and most of the ciliæ were still inverted. The proposed operation was to remove the anterior tarsal margin of each upper lid, with all of the hair bulbs. James H., æt. 33, was taken before the class at St. Vincent's Hospital. Three drops were instilled into the eyes every three minutes for twenty minutes, and besides this eight drops were twice injected subcutaneously at the outer canthus of the right eye. A narrow scalpel was thrust into the edge of the lid for the purpose of splitting it, but he kicked, bellowed and struggled so terribly that the operation had to be completed under ether.

CASE III. Removal of portion of prolapsed iris, in case of ulcerative keratitis. W. S. æt. 16 years, very strumous; has had "sore eyes almost since infancy." Three drops were put into the eye every three minutes. When the first application was made the orbicularis resisted one's efforts to open the lids, but on each renewal less resistance was experienced until it was found easy to drop them into the eye, even without touching the lids. She did not flinch when the speculum was applied, but when the iris was seized with the forceps she complained very much, but we succeeded in excising it.

CASE IV. John E. S., æt. 34, a traveling salesman. Very small chalazion at tarsal margin of left upper lid. Two drops were used in the eye every three minutes, and dropped on the outside of the lid while he occupied a recumbent position. He was told that while this would not cause the operation to be painless, it would very much mitigate it by benumbing the parts, thereby removing about three-fourths of it. When first dropped into the eye he flinched and contracted the lids greatly, but after a few additional applications, he soon suffered its use without any resistance, and in twenty minutes the pupil was widely dilated, and the eye much more widely open than the other. Knap's lid clamp was then applied, and the little growth was removed while the patient puffed and blew like a porpoise. When the operation was finished he remarked that it was a great blessing that a remedy was used which removed three-fourths of the pain, as the remaining one-fourth nearly killed him.

CASE V. Emma S., æt. 17. Strabismus convergens of over six lines, requiring operation on both internal recti. The usual 4 per cent. sol. was used every three minutes for nearly forty-five minutes until the pupils were widely dilated, and the resistance of the orbicularis most thoroughly overcome; but no sooner was the conjunctiva grasped, than she struggled and complained most terribly, and this she kept up during the whole length of the operations on both eyes, and she vomited during the following afternoon and night.

CASE VI. William W., æt. 50. This was a patient on whom an extraction for cataract had been

made six months before, from the right eye, which had been followed by irido capsulitis and closed pupil; in the left he had cataract and iridodonesis. The operation was an artificial pupil in the right, and a preliminary iridectomy in the left. When first operated on for cataract he contracted the lids so tightly when the speculum was placed in the eye, that it was removed until ether was administered and the operation finished. This was an excellent case in which to test this remedy. The drops were applied every three minutes for over twenty and until the left pupil was well dilated. After the third time of using them he became much less touchy about his eyes, and very soon they were used without any winking at all. The speculum was applied to the left without any resistance on his part, but when the conjunctiva was grasped by the fixation forceps he winced considerably, and when the iris was seized he jerked, and tore it loose from the forceps; again was it grasped and cut off while we scolded him most unmercifully for his nonsense. More drops were used on the right eye when a similar experience was again met with, and the operation was finished with De Wecker's scissors.

Case VII. Patrick H. B., æt. 48, machinist. Removal of small piece of steel from the cornea. Two drops instilled every three minutes for fifteen, and body dug out without any difficulty. On questioning him he said that it did not hurt much; hardly as much as usual. We must remember, though, that machinists are so in the habit of having bodies removed by their fellow workmen, that these are not of the best class to test the virtues of this remedy.

Case VIII. Re-insertion of style in nasal duct. Mrs. C., æt. 49, had submitted to the operation for stricture of the duct, and had had a style inserted without taking an anæsthetic, two months previously, and had made but little complaint during the operation; but on its removal, every ten days or two weeks, she always said it pained her on its insertion. This was doubtless owing to the peculiar curvature of the passage. The drops were used very freely for the usual length of time, and no force was exerted in replacing said style, but she experienced no difference whatever in the slight pain usual to its insertion on former occasions.

Case IX. Removal of half of the prolapsed iris in lacerated wound of the cornea. John H., colored, æt. 52, was wounded with a brickbat two weeks before, and had been kept quiet with iced compresses and atropia locally, before I saw him. The eye was still much inflamed, and quite a chemosis was present. The cocaine was used every three minutes for twenty minutes, when the speculum was inserted. He flinched but little during this part of the operation, but when the iris was removed he complained very much.

Case X. Enucleation of the left eye-ball for sympathetic irritation. Ellsworth C., æt. 20, lost the vision in the left sixteen years ago, from wound through cornea and ciliary body. Three weeks ago he commenced losing the sight of the right eye. He was urged to have the eye removed under the influence of the cocaine; was told that though it would not entirely relieve pain, it would very much mitigate it. He was treated with it every three minutes for over half an hour, but he declared that he did not feel any difference in the eye, though it was noticed very early that after the use of the first few drops he ceased to contract the lids, and the medicine was applied without having to open the eye with one's fingers. He made no complaint when the speculum was adjusted, shrank a little while fixing the eye, and urged me to hurry up while the muscles were being taken up. The operation lasted two and a half minutes, and when completed he asked if the eye was out, and said that "It did not hurt half as badly as he had expected; but as he had never even had a tooth pulled, did not know how much such cases did usually hurt one." He seemed dazed during the operation, and doubtless would have acted similarly had no cocaine been used.

From the above experience it is my opinion that as an anæsthetic in eye operations this alkaloid has been exceedingly overrated, and that many of the cases which have been reported would have done almost as well had it not been administered. I have been strikingly strengthened in this opinion quite recently, for while waiting for the arrival of the medicine (which took nearly four weeks to procure) an unusual number of cataract cases presented themselves for treatment, all of which were operated on without any anæsthetic whatever, and, doubtless, had it been used in these, as was my intention, I also would have gone into ecstacy concerning its "wonderful" efficacy. I have used it in a very large number of cases where my object has been simply to get a view of the whole of the fundus of the eye, and for this purpose it seems to have no superior, as its effects pass away more rapidly than even when homatropine is used; but it does not seem to sufficiently relax the accommodation to justify its use in the exact measurement of anomalies of refraction. While using it for the purpose above-mentioned, I found that its effects upon the palpebral portion of the orbicularis are much greater than are those of other mydriatics, as in a very large majority of the cases in which it is used the eye is opened much more widely. Again, its action upon the sphincter of the eyelids points to its desirability in cataract extractions, as they undoubtedly tolerate the speculum much better than when it is not used. I would also suggest its use in tonic blepharospasm. My next door neighbor, who is a throat specialist, tells me it has done wonders with the nasal mucous membrane. So, taking these things into consideration, it seems to be a valuable agent in a limited field. It is to be hoped that the newspaper puffs which it is receiving on every hand may continue, as by this means we can tell our patients that we are going to resort to that "precious" remedy concerning which they have, doubtless, read so much; then many will submit to operations without the aid of ether who would never have consented before. Faith in the remedy will have its effect on others, and by the time this has found its true position, in all human probability something else will arise to take its place.

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ON THE [1]UNITY OF POISON

IN SCARLET, TYPHOID, AND PUERPERAL FEVERS; ERYSIPELAS, DIPHTHERIA, SORE THROATS; CERTAIN FORMS OF DIARRHŒA, AND ALLIED AFFECTIONS; PLEURISY, PNEUMONIA, PLEURO-PNEUMONIA; AND MANY OTHER AILMENTS USUALLY CONSIDERED TO BE SEPARATE AND ENTIRELY DISTINCT DISEASES.

BY DR. G. DE GORREQUER GRIFFITH,

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"If a man be content to begin with doubts he shall end with certainties."—BACON.

"All scientific novelties are subject to inconsiderate criticism."—PASTEUR.

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From the facts connected with these [2] affections—clinical and pathological—embracing as they do, the etiology, the history of origin, progress,[3] interchange, and inter-communicability, mode of propagation, hybridity, and transmutation, we cannot avoid arriving at the conclusion that these ailments may be generated *de novo*, and from one common source, as well as by direct communion with one already suffering; or by indirect, through some secretions, excretions, or some thing, or person, that has been in contact, or communication with the sick.

That there are two forms of scarlatina is distinctly manifest—a fact thoroughly established on evidence from which there is no escape. There is—1st. That contracted from a scarlatina patient, or from some one or thing that has been in contact with him, or in communication with him. To this I give the name orthodox scarlatina. There is—2nd. That generated *de novo* from blood poisoning, such as occurs in puerperal patients, and in surgical cases (the "surgical scarlatina" of Sir James Paget) from deleterious matters absorbed or otherwise passed into the blood, and thence into the tissues of the body more or less generally; or from noxious drains, sewers, imbibition, or ingestion of pernicious articles of drink or food—such as tainted water, milk, cream, decomposing animal or vegetable substances. To this form I have given the name toxæmic, to distinguish it—as regards origin—from the orthodox. As an example of what I mean I shall give the following particulars:—

At one of the meetings of the Harveian Society the President brought forward a case which he termed "simulative scarlatina from eating tomatoes," in which occurred all the symptoms of scarlatina, except that there was no desquamation; and for albumen in the urine no search was made.

In passing, however, I may observe again—I have made the same remark elsewhere—that desquamation is no proof whatever of the existence of orthodox scarlatina, and its absence no disproof of orthodox scarlatina having attacked a patient; for, high temperature of the skin, arising from any causes—and they are various, setting scarlatina aside altogether—will occasion such rapid cell changes, that the surface cells will be thrown off in larger or smaller quantities, and, according to the height to which the temperature has run, will they be shed wholesale, being effete. Thus arises that process which we call desquamation. In some undoubted cases of scarlet fever there may be only slight exaltation of temperature, or so transiently may it take place that it escapes detection; or it may even be altogether absent, owing to the great and sudden depression of vitality produced by the virulent character of the virus, or by the largeness of the dose taken into the system, or the suddenness of its invasion. With this absence of high temperature desquamation will also be absent. Nor is albumen in the urine a pathognomonic sign of scarlet fever poison pervading the system. In how many conditions and affections of the body other than those demonstrating the existence of scarlatina will not albumen be found? "There is nothing unusual in the fact that inflammation of an important surface should be attended with albuminuria," says Mr. Jonathan Hutchinson. (See *Brit. Med. Journal*, May 3, 1879, page 665, under head of Discussion at Medico-Chir. Society on Report of the Committee on Croup and Diphtheria.)

Here—in the person of the patient brought before us—was undoubtedly a case of "toxæmic scarlet fever," symptoms generated, as far as could be traced, by the eating of tomatoes. No source of contagion or infection having been discovered or being discoverable, no other source of origin, these fruits were apparently the *origo mali* of certain chemico-physiological changes in the system, which were sufficient to produce a general poisonous effect that demonstrated itself in certain physical changes, phenomena or symptoms—the majority of which, or the entire in the aggregate, being such as to lead the observer to say the patient had got scarlatina.

Bearing upon this case, I would adduce an instance wherein the symptoms of scarlatina were all so marked that it is specially applicable, and fully corroborates the President's in showing that ingested materials will at times induce symptoms so closely resembling orthodox scarlatina as to be undistinguishable. And here I would ask, if distinguishable, how, then, does the attack differ from orthodox scarlatina, *i. e.*, that which has been contracted from a scarlatina patient, or some one or thing that has been in communication with him? *The difference lies only in origin!* In my case the symptoms rose from eating very high game, and though not proceeding from eating tomatoes, it is relevant—1st. Because in all points it

[1] By unity is meant not that the poison is always the same, but that the one poison—the one *origo mali*—whatever it may be, will originate several so-called different affections.

[2] I had at first placed at the head of my paper only the first four diseases (keeping to the conventional word).

[3] "Erysipelas is a specific disease, due to the presence of some blood poison that has probably been introduced into the body from without; it is intimately allied with other blood poisons, such as are found in scarlet or puerperal fevers, or septicæmia in any of its forms: for their poisons seem to be convertible."—Bryant's *Surgery*, second edition, vol. i, p. 44.

was one of "simulative scarlatina"—a name I was the first to use some years back, but for which I now prefer to use the term "toxæmic scarlatina;" 2nd. Because it arose from a similar, though not the same cause, ingested material; and thus, thirdly, was an answer to the query propounded,—Were the symptoms in the instance before us due to eating one of the solanaceæ order?—inasmuch as it shows that causes other than *the active*[1] *principle of that order* produce such symptoms, even while admitting that the active principle of the tomatoes may *per se* at any time, under any circumstances at all events, set up toxæmic scarlatina, and may in this particular patient have directly produced the symptoms and been the direct exciting source of the illness.

A second case which I would adduce bears strongly on the same; so strongly, indeed, that though having published it in 1875, I will quote it, as it also is to the point:—

1st. Because in symptoms it was identical; 2d. Was what would be termed "simulative (toxæmic) scarlatina;" and 3d. Showed that these symptoms may arise even from *animal* poisons passing into the system *in a way other than by the mouth;*—viz.: by the womb, in contradistinction, though not in opposition, to the tomatoes case.

Thus again answering the question directly by showing that the "simulative (toxæmic) scarlatina" symptoms are not *necessarily* dependent upon the ingestion of solanine, since they will be produced by animal irritants and poisons absorbed not through and from the mouth and stomach alone, but in other ways also.

I will here relate the case—a typical one, and such as has come before every medical man.

The conditions alluded to, and which are identical with scarlatina—are the same I now consider—have their origin not from orthodox scarlet fever contagium, that is, from a scarlatina-infected person, or some one or thing belonging to such a patient—there never having been any exposure to scarlatina in that way—but from the putrescing and offensive matter in utero and vagina, the resulting pyrexial action being indicative of the concealed cause, the latent mischief, that, without careful observation, might pass unobserved.

With the train of symptoms to which I refer, who practising obstetrics is not acquainted? The chilliness and shivering, the sickness, the dreadful headache, and the congested eyeballs from which the paining light must be veiled, or even shut off altogether; the sore throat, crimson with inflammation; the peculiar tongue; the hot, pungent skin, that so rapidly sends up the mercury, and is *dyed with the unmistakable rash*, with which there is a tinge of jaundice-yellow, here and there more noticeable; the delirium; the oppressed heart and breathing; and later on the diarrhœa. What are these symptoms to be termed? Not *simulative* scarlatina, surely, since they are not simulative at all, but are exactly the same as obtain in scarlatina contracted from a previously infected person or source—orthodox? What, then, are they? Symptoms of what, for contradistinction's sake, I term "toxæmic scarlet fever," but assuredly as true scarlatina as the other form—the orthodox—though not generated in the same way, but generated *de novo* in the woman herself, and the outcome of the condition of blood poisoning existing, *not* from the introit of a *specific virus*, but of elements given off in utero or vagina—not, it may be, noxious when given off, but becoming so subsequently from morbid changes—and, when noxious, taken into the system; a blood-poisoning, I repeat, the result solely of the putrefying animal matter in utero or vagina, and of unhealthy changes, taking place locally first, in the generative tract and system, setting up local mischief primarily, and then developing and evoluting through the entire body of the puerpera, till all the symptoms I have portrayed have made their appearance. In a scarlet-fever epidemic, such a group of symptoms happening to the lying-in would be set down to scarlet-fever poison, contracted from another affected with it, or from some one or thing belonging to, or that has been in contact with, such an infected person; for it is not yet generally accepted that scarlatina can commence *de novo* in the manner I describe. But these symptoms will be found where no previous case, or contagium, or source of origin other than what I name, has existed.

What, then, are those phenomena? I gave them the name of bastard or simulative scarlatina when I first wrote on this subject, not at that time considering them true scarlet fever; or knowing that the ailment could be so produced; or, indeed, be produced *de novo* in the ways that further researches have shown me it can. True—orthodox—scarlatina I consider to be that form contracted from a previously existing case, or from some one or thing connected with, or which has been in some way or another brought under and near the influence of the scarlatina virus existing in a previously infected patient. Bastard, or simulative scarlatina—toxæmic—I consider to be those scarlatina manifestations or appearances heretofore deemed mere resemblances—a view I also then took, but which I now know to be not resemblances, bnt the actual manifestations themselves of toxæmia set up otherwise than from orthodox scarlatina, differing only from the orthodox form in that they originate *de novo* from such sources of contamination as may occur in the lying-in; or from vitiated water, milk, cream, or the taking in of "high," *i.e.*, putrescing articles of diet,[1] or decomposing drinks, such even as milk which has begun to undergo deterioration, it may be simply because it has been long "drawn;"[2] or it may be because it

[1] Belladonna, it is well known, produces scarlatinous efflorescence, etc., etc.

[1] Upon this Dr. Carpenter wrote very clearly in 1853, in the *Brit. and For. Medico-Chirurg. Review.* See his article, "Predisposing Causes of Epidemics."

[2] The following remarks will explain what I mean when I say that even milk undergoing deterioration will produce scarlet fever:—On Thursday, January 30, 1879, at the meeting of the Metropolitan Counties Branch of the British Medical Association, Mr. Gay, in a paper on "Milk as Food for Man," said:—"In the absence of any positive knowledge as to the source of the various exanthemata—measles, scarlatina and the like, characterized by fever, and without doubt related to some form or forms of blood poisoning, disorders that abounded mostly after dentition, and when milk was often the predominating, if not the sole article of food, was it unreasonable that we should turn with suspicion to *those unwholesome ingredients of decaying milk* for their explanation?" He continues—"Milk when once exposed to the air and rest, underwent a series of chemical changes through the influence of external and internal agencies which

has been allowed "to stand" in a place, and under circumstances of temperature,[1] etc., which would permit or hasten unwholesome changes,[2] the milk being at the time not unpleasant to the ordinary taste, nor even at all perhaps perceptibly altered in flavor.[3] I now recognize these symptoms, arising from causes other than the infection or contagion derived from a previously infected person or thing, to be as true scarlatina, and capable of becoming in its evolutions and its transmigrations from one to another, as infectious and contagious as the other form. In confirmation of this view I would name that Sir Wm. Gull has stated his belief that diphtheria may "*commence locally*, forming a poison which will propagate itself;" and he has "drawn a distinction between diphtheria and diphtheria poison, regarding the disease as beginning with a poison, but *sometimes as commencing locally, and forming a poison which may propagate itself.*"—(*British Med. Journal*, May 3, 1879. Page 666.) This bears me out in the contagious and infectious character of toxæmic scarlatina; and the evidence of Burdon Saunderson, which I have quoted in my former paper, to the effect that an inflammation may begin as "simple" but go on progressively to what would be termed specific, also is corroborative. Again, we know that a benign may be converted into a malignant ulceration, progressively, under continued irritation.

And it will be found that scarlatina generated *de novo* from blood poisoning, or effected in any other way than that *usually accepted as being the only way* in which scarlatina is generated and propagated—viz.: from a previously scarlatina-infected person or thing—is infectious and contagious alike with the other form. I do not now consider *that* to be bastard or simulative scarlatina which I formerly described as such, but look upon it as true, *generated differently, propagated similarly*. I was first led to these conclusions by a case of auto-genetic poisoning, which was under my care in 1875, in which scarlet fever symptoms obtained, but which, recognized to be wholly the consequence of a morbid condition of the uterine system and its excretions, in fact, begotten and conceived *in utero* from putrescing matters therein, I treated accordingly, by grasping the soft spongy uncontracted uterus with my hands, compressing it, expelling thereby the retained and offensive clots, washing out the vagina and uterus by means of Higginson's syringe—Condy and warm water being used every two hours till the lochia became quite inodorous. Had I, however, seen the patient for the first time when those scarlatina symptoms were fully developed, and had I overlooked her puerperal state and the condition of the uterine system, I might have erred in supposing I had to deal with scarlatina from a previously infected person or thing, have overlooked the *de novo* origin of the case—the latent uterine mischief—have neglected the ablutions and cleansing processes, have treated the ailment in the orthodox fashion as being contracted from previous scarlatina infection, and no doubt have lost my patient. But being alive to the puerperal facts and bearings to which I have drawn attention, I at once proceeded as I have described, and the correctness of the diagnosis was evinced by the immediate improvement in the lady, commencing as it did from the very time I got rid of the offensive clots and washed away the noisome lochia. Within twelve months I had three cases of puerperal blood-poisoning, in which not alone was scarlatina rash well marked, bnt also the other symptoms of scarlet fever. I had learned from my first case valuable knowledge by which I have profited, and these later patients I accordingly treated as suffering from toxæmic scarlatina. Some time ago I had, in consultation with Dr. Joliffe, of Shepherd's Bush, a case of typhoid fever, in which relapse occurred while convalescence was advancing and desquamation—as truly marked as in what would be termed a typical case of scarlatina—was actually taking place. In the same house where lay this sick mother some of the youngest members of the family had previously been affected with scarlatina, others having suffered from what is termed scarlatina throat, while others again were at the time complaining of the same, the cause of the outbreaks being, as far as could be discovered, the defective drains, which allowed regurgitation of noisome smells and vapors into the house.

Since 1875, when I first entered upon the study of this question—the unity of poison and differentiation of resultant phenomena which we call symptoms—not because of any difference in the poison which

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ensued in its degradation and decay, its last stage being that of a limpid fluid, mainly composed of water, salts and acids, amongst which figured the lactic, butyric, caprionic, and capriolic. To this downward career it was carried by becoming the habitat of broods of vibriones of different kinds, and, according to Professor Lister, of a certain bacterium—bacterium lactis—and by certain carpetings of its surface by oidium and other varieties of mould."

[1] "M. Fauvel, of the Paris Chemical Laboratory, has discovered an additional danger to that abominable invention, the baby's feeding bottle. Of 31 feeding bottles taken from various creches in Paris, 28 contained both animal and vegetable life, the milk remaining in all smelt badly, was acid and half coagulated; the globules were deformed, and numerous very lively bacteria, along with some vibriones, were present. On cutting open the caoutchouc tube through its length, coagulated milk, with small organisms, was met with; but a still more important fact was the presence in the nipple of a mass of vegetation of cryptogamic nature. Some of these bottles had been washed and were ready for use."

[2] The lactic fermentation has relations to a small oval bacterium, which hence has been called by Lister, bacterium lactis.

[3] These deleterious, if not absolutely poisonous changes, in a fluid introduced into the system and taken up into the blood, show how milk and cream, and substances or fluids with which they are mixed and combined, can and will prove a source of originating blood-poison which, in different periods of life—cycles of age—and under different circumstances, will declare itself in symptoms at one time, or in one person, of one or other of *so-called different* AFFECTIONS; at another time in the same person in a different form or set of symptoms, or at the same time in different people in the different manifestations to which have been given specific names—scarlet fever, typhoid, puerperal fever, diphtheria, erysipelas, etc.—for the purpose of distinction and of conveying to others what we mean, and which I look upon as phases of the evolution of the one poison.

I would call to mind how often the puerperal woman is dosed with gruel made with milk—that has in many cases been "standing" for some hours, and it may be in the puerperal room itself, wherein the atmosphere is not always, by ignorant people, kept the sweetest,—on the supposition that milk creates milk; how the children, and husband, and household often partake of the same milk; and how vitiated (as shown in Mr. Gay's paper) even good milk becomes by "standing," how much more by standing in the atmosphere of the lying-in chamber; and how readily this one fluid becomes an originating virus, a one *origo mali* in a household, or circle of people poisoned with it, amongst whom there would be *from this one source* a differentiation and divergence of results, of phenomena of the poisonous action, of symptoms directly consequent, so that one or more would be said to be suffering from scarlatina, others from typhoid and diphtheria, or one of the other generally supposed specific and distinct ailments of which I write, the lying-in mother being attacked with what would be termed puerperal fever: all the differentiations resulting not from a difference in the poison, but of the persons brought under its influence.

The experiments of Pasteur on chicken cholera are well-known. In hope of diminishing the infective power of this organism, *he grew it in oxygen* for a long time, and the found not only that *it produced a modified disease*, but that this attack in most cases protected the animal from the effect of the organisms *in their most virulent state!*—*Brit. Med. Journal*, December 31, 1881. Page 1062.

may be the *origo mali* of so-called various diseases, but differentiations dependent upon the media through which the poison passes, or upon which it falls, the evolutions and developments of that one poison being marked by new phases, new manifestations to which rightly we should not apply the expression separate diseases, but rather call them simply what they are, the various expressions of the evolutions of that unity of origin whatever it may be, which has set them all agoing—since, I repeat, I first entered upon the study of this question, my researches have made clear to me what before I had not recognized; and out of much mistiness, obscurity, and chaos of facts, ideas and thoughts, clearness and definiteness have evolved in my mind, so that what was to me—as to every observer in a new path—at first vague, indefinite, unshaped, has become certain and embodied in realities. What at one time I feared to be incapable of proof has come to be more and more provable, till at length it has become proved; what at one time from former teaching and consequent prejudice seemed to me to be false, has gradually opened out to me as true, till it has come to be established in my mind as a great truth.

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APPENDIX.

It is with great pleasure that I embrace the opportunity of presenting through the medium of the JOURNAL the theory of "the unity of poison," advanced by Dr. Griffith, who has courteously permitted me to republish his views, as they have appeared from time to time in the columns of the Glasgow *Medical Journal* and *Midland Medical Miscellany*. The author has kindly consented to allow me to comment upon the views expressed in the foregoing pages, and I shall therefore make such comments as are suggested to me by their perusal.

While accepting, in the main, the arguments advanced by Dr. Griffith, and believing them to contain the germ of a great truth, there are some points to which I think we may take exception. In the first place (such eminent authorities as Paget notwithstanding), the propriety of dividing scarlatina into two distinct forms, such division being dependent upon a hypothetical difference in their etiology, is questionable. The occurrence of efflorescence, pyrexia, desquamation, and albuminuria, one or all, consequent upon the ingestion of tomatoes (which ordinarily are a healthful article of diet), does not constitute scarlatina, any more than pain in the side, fever and cough, one or all, constitute a pneumonia, but such scarlatinoid symptoms simply indicate that the poison, whatever it may have been, has the same physiological action as the *materies morbi* of scarlatina. The similarity of symptoms does not indicate a similarity of poison, *but that there is some physiological co-relation due to the fact that the effects of both poisons are manifested through their influence upon the same portion of the economy, viz.: the sympathetic ganglia and trophic nerves.* The "toxæmic scarlatina" is probably not scarlatina at all, being simply a collection of morbid phenomena due to profound disturbance of the sympathetic ganglia, by a powerful organic poison, and perhaps influenced greatly by idiosyncrasy. Some persons cannot eat shell-fish without a severe resultant urticaria, not because shell-fish *per se* are poisonous, but because the sympathetic system in such individuals is intolerant of that particular article of diet. As Dr. Griffith states, belladonna produces an efflorescence resembling scarlatina. Quinine, copaiba and other drugs will produce an eruption, and in the case of quinine especially, usually in cases of idiosyncrasy. The specific poison of syphilis produces an eruption resembling measles, which is entirely independent of cell proliferation. This "roseola" is transitory, and produces no permanent change in the tissues affected. We could hardly accept these phenomena as evidences of their identity with measles or scarlatina. But there must be some explanation of their similarity, and that is the fact that *certain poisons act upon the system through the medium of some physiological disturbance of the sympathetic.* The medium being the same in all cases, a similarity of the resultant morbid phenomena might reasonably be expected. Perhaps, as the author states, the difference between the different series of phenomena, as developed particularly in the two forms of scarlatina, lies only in "origin," but that is often a very wide difference. The only essential difference between narcotic and uræmic coma is in their origin, yet they are not the same. It is the peculiar origin of each that constitutes the disease.

The author is quite correct when he asserts that the absence of desquamation and albuminuria is no proof that scarlatina is not present in a given case, but at the same time that fact could hardly be advanced as evidence of the unity of origin of "orthodox" scarlatina, and the systemic disturbance caused by the ingestion of tomatoes, or too high game. Even if both phenomena were quite frequently present in the so-called "toxæmic" scarlatina, they could be quite readily explained, without recourse to the theory of "unity." Desquamation might readily follow any peripheral irritation which interfered with the nutrition of the skin, and as is well known, the same factor of peripheral irritation would explain the albuminuria. As has been shown by Mihrau[1] a full hot bath will produce it. Irrespective of the cause, a vicarious strain upon the secretory structure of the kidney may be quite as effective in the production of albuminuria, as may the selective action of some poison.

While I am heartily in accord with the author in his opinion as to the co-relation of the poisons of disease in general, and especially with the explanation of such an interdependence based upon evolutionary laws, I incline to the belief that the tomato argument is not of great weight. It is hardly safe to attempt to support so radical a position, with so few cases. Again, it must be acknowledged, that it is only in a very small proportion of cases that we can trace our cases of scarlatina to their sources of contagion. In the instance of both the tomato and game cases cited by Dr. Griffith, a mere coincidence is highly probable.

[1] These de Paris, 1882.

In a case of puerperal scarlatina, (even if the occurrence of a "toxæmic" form of scarlatina be admitted) it would be absolutely impossible to differentiate it from true scarlatina with a complicating septæmia, which is more than likely to occur, and which in my own estimation, is the explanation of the fatal character of scarlatina in the puerpera, whose secretions become septic as soon as the woman is attacked by the specific disease. The case of "toxæmic" puerperal scarlatina cited by the author, is an ideal picture of scarlatina in the puerpera, complicated by septæmia. I can see no logical reason for the inference that such a case of scarlatina is caused by septic infection.

If such were the case, the disease would occur more frequently in our hospitals, where septæmia is so frequently seen. Even from the evolutionary standpoint, (which in the basis of Dr. Griffith's views) it would be very difficult to explain so sudden a transformation of simple septic matter into the *materies morbi* of scarlatina, as such an occurrence in the puerpera would imply, particularly in cases of autosepsis. The law of evolution is certain, but it is as gradual as it is certain and irresistible. The train of symptoms referred to by the author may not be rare in London, but they are certainly rare in this country, as I think will be conceded by our physicians of extensive midwifery experience, and their rarity is still greater, if cases of indubitable contagion be excluded. As for the *de novo* origin of scarlatina, it can hardly be possible under the circumstances mentioned by Dr. Griffith.

But can scarlatina ever be developed *de novo?* I am quite ready to believe that it can; *but such development must occur by a normal and gradual process of evolution, not suddenly by autogenesis through the medium of a simple septæmia.* Scarlatina has its own peculiar and specific germ, *materies morbi*, or whatever we may choose to term it, and this cannot develop spontaneously in the true sense of the term, *but in the presence of an appropriate environment, the minute organisms (or minute particles of organic matter, if such be the term preferred) everywhere present in the atmosphere, may by a slow and certain process of evolution, undergo a transformation, and acquire new properties by virtue of which they become infectious, and capable of producing disease of a specific type.*

In the same way, organisms of a higher specific type may retrograde, and become harmless. These views may serve to explain the gradual rise and decline of intensity of epidemics. The increase and decrease of virulency of specific diseases may thus be due to imperceptible changes of environment. The propriety of this adaptation of the great truths of evolution to the explanation of the origin and natural history of disease, will of course be disputed by all who oppose evolution as a whole, but if the theory of evolution be correct, and the germ theory of disease be proven, the co-relation of the two is indisputable. *Accept evolution, and we must accept the evolutionary theory of disease.*

It is extremely doubtful whether the processes of simple fermentation and putrefaction are capable of developing the *materies morbi* of specific disease (that of scarlatina for example) an occurrence which to Dr. Griffith, appears highly probable. Septic materials, when introduced into the stomach, are speedily shorn of much of their noxiousness, and the results of such ingesta are as a rule, chiefly local. The gastric juice is the great physiological antiseptic, and constitutes an almost impenetrable barrier to many noxious materials of an organic nature. That simple lactic fermentation is capable of developing a systemic poison of a specific character, I am hardly prepared to believe, and surely, we have very few facts to support such a theory. Fortunately for us, the gastro-intestinal tract is very intolerant of many of the injurious products of fermentation and decomposition, and this intolerance results in their elimination, before they have had an opportunity to act injuriously upon the general nutrition.

In confirmation of his views the author quotes Sir Wm. Gull as stating "that diphtheria may commence locally, forming a poison which will propagate itself," he having consequently come to recognize two distinctive forms of diphtheria.

Now I am under the impression that it is a well recognized fact that diphtheria may commence locally by inoculation, and it is supposed by the majority of observers, that such commencement can occur in no other way. There is, however, no clinical distinction as far as known, between the cases resulting from inoculation, and those from constitutional infection. Gull's theory does not imply inoculation, but a spontaneous development of the diphtheritic poison. This I believe may occur, but in a manner so gradual, that it is extremely difficult to say where a simple process terminates and a specific begins. In accepting this view, however, I can see no grounds for differentiating the disease into two distinct forms. The degree of severity may differ in different cases, but the type of disease remains unaltered. Again, admitting Gull's theory as to the origin of diphtheria to be correct, it is difficult to understand how it can be adduced as evidence in favor of simple lactic fermentation developing scarlatina.

That the noxious vapors and gases of the sewer are capable of producing a variety of diseases, is well known, but it is quite generally accepted that such sewer emanations contain the distinctive germs of these different affections. There would seem to be, however, many peculiar cases which are hardly explicable upon the ground of a specific germ infection, and it is just here that an acceptance of Dr. Griffith's views is almost unavoidable, at least, in so far as his doctrine of the interdependence of the various poisons of disease and their unity of origin in certain instances, is concerned. As before stated, however, I believe that the transformation of the one original poison into a number which affect the human system by the production of various so-called specific disorders, must occur through a gradual process of evolution, and not by a sudden metamorphosis such as must occur if the author's deductions regarding the origin of his cases of scarlatina be correct.

There are many striking facts bearing out the theory of "unity of poison." The close similarity between

certain malarial fevers and typhoid, between typhoid and typhus, and the marked resemblance of a certain febrile condition attended by diarrhœa (which we are so fond of attributing to sewer gas), to typical typhoid, may be explicable from the standpoint of unity. There must be something more than professional stupidity to account for the difficulty experienced in differentiating certain diseases, and which almost brings the diagnosis down to the level of guess work. What peculiar combination of circumstances relating to the atmosphere, constitutional condition or hygienic surroundings of the people, determines the prevailing epidemic influence in any given locality, is unknown, and probably will always remain so.

The possibility of the spontaneous origin of specific disease has ever been a source of controversy, but that such an origin is demonstrable will, I think, be evident to anyone who carefully studies some of the more recent epidemics. The late epidemics of typhus fever in New York are excellent examples, and the preponderance of evidence would seem to show that the epidemic of cholera now declining in France, was due to local causes, and consequent spontaneous development of the disease. It is only by a recognition of the *de novo* origin of diseases of a specific type that modern sanitation can accomplish its object.

A very interesting instance was recently reported in the N. Y. *Med. Record*, by Dr. L. J. W. Lee,[1] which would appear to be strong evidence in favor of the theory of unity. In this instance, among ten cases of disease in one house, variola, varicella, measles, typhoid, and remittent fever were said to be represented. As might be imagined, the theory of unity of poison is not an easy one to support by actual demonstration, *e.g.*, it would be difficult to prove in any given instance, that the emanations from the sewer, did not contain germs derived directly from cases of specific disease.

The theory of unity as I understand it is in nowise opposed to the germ theory, and the same may be said of the theory of the spontaneous origin of specific disease. Each may take as its point of departure, the harmless organisms universally a component of the atmosphere. These organisms, from causes not at present definitely known, but unquestionably by a gradual process of evolution and adaptation to environment, become metamorphosed into poisonous germs, which, when introduced in the human system, will produce disease. An example of this is quoted by Dr. Griffith,[2] from Paget.[3] In this instance, occurring on board a vessel at sea, a sailor wounded his hand with a beef bone, (the condition of the bone is not stated, and there is reason to surmise that it may have been poisonous from decomposition), the injury being followed by fatal cellulitis with axillary suppuration. Subsequently, cellular or erysipelatous inflammation in various forms, ran through the whole ship's company, and terminated in an epidemic of mumps. The whole course of disease was over a month, the ship being at sea the whole time, and during the greater part of it, in the severe cold of a southern winter. Here, the first case in the presence of the crowd poison always present on board ship, afforded a laboratory for the formation of an intensely infectious poison, which in consequence of the intimate relations existing between the members of the ship's company, had no obstacle to its dissemination. Whether the parotitis was the genuine "mumps" or not is questionable. It may have been, in a mild form, the same inflammation of the parotid, which is liable to occur in pyæmia, septic peritonitis (so-called), and other toxæmiæ. A case is cited by Dr. Griffith from his own experience, which, although interesting, is open to the same objections offered to some of his other evidences in favor of "unity of poison." In this case a patient convalescing from typhoid fever suddenly developed symptoms of scarlatina. Cases of true scarlatina, or of scarlatinal sore throat, were present at the time, or had been present in the house. "The cause of the outbreaks was, as far as could be determined, the defective drains." Now, these causes probably did result from the same cause as far as the sewer exhalations were concerned, but, unfortunately, sewer gas is a compound and decidedly complex substance, and in this case, as in others, may have contained the germs of several specific and distinct diseases. I wish that it were possible to accept such evidences of "unity of poison" as indubitable, as it would greatly simplify our studies of etiology. As I have already remarked, I believe it extremely difficult to support the theory by actual demonstration, but, nevertheless, the known facts and powerful evidences of the theory of evolution in general, associated with our knowledge of the germ origin of disease, render the theory of "unity of poison" one of the most logical explanations of the causation and differentiation of disease, yet advanced. I believe, however, that the common origin of the poisons of disease is not a poison, but a harmless microscopic organism.

125 State street, August 5, 1884.

[1] Influence of diathesis upon contagion. N. Y. *Med. Rec.* Vol. xxv, No. v, p. 84.

[2] *Midland Medical Miscellany.*

[3] Paget, Surgical Pathology, 3d Ed. P. 373.

MEDICAL PROGRESS.

OBSTETRICS AND GYNÆCOLOGY.

MASSAGE IN GYNÆCOLOGY.—Dr. Prochownik has recently read a paper on this subject at the Versammlung Deutscher Naturforscher und Aerzte at Magdeburg, a translation of which is given in the *Medical Chronicle*. He refers to the doubt and reserve with which this reintroduction of massage into therapeutics was received, and acknowledges that its results were certainly for a time exaggerated, and that in consequence it fell into the hands of the humbler sort of practitioners and of quack specialists. But already the results of massage in surgery, and even in gynæcology, in the hands of some of the operators by profession, have been so striking that the subject urgently challenges an experimental investigation. As one who has freely used it for two and a quarter

years in gynæcology, he sets forth a number of exactly formulated and easily discussed propositions, viz.:

1. The employment of massage in gynæcological therapeutics aims at the acceleration of the absorption of pathological products, the stimulation of the circulation, the stretching of shrunken and cicatricial tissue, rarely the production of contraction. Parallel with these objects the following diseased conditions come under consideration as the most suitable for treatment by massage, viz.: Transudations, exudations, metritis, subinvolution with loss of tone, cicatricial stricture, and contractions with their resulting conditions, rarely neoplasms.

2. Massage should never be employed except when all other therapeutic agents have failed. Every course of treatment by massage is tedious, often lasting for months, and it is almost always painful. Patients decide more readily to submit to this when other remedies have failed.

3. Another previous condition to the practice of massage is an exact differential diagnosis, the result of careful, combined examination. We must not hesitate to make an examination under an anæsthetic, in order to make the diagnosis clear and definite.

4. Gynæcological massage divides itself into an external and an internal. External massage is very little used, but it is advisable in most courses of treatment to begin with it, if only to test the amount of sensibility. The circumscribed employment of external massage depends upon the fact that only in a small number of patients is it possible by external manipulation to produce any effect upon the organs of the true pelvis, because every kind of massage requires a certain amount of resistance for its efficiency. The employment of massage as a test of sensitiveness is suitable for most patients, but not for all; there are women who are completely intolerant of internal measures, and who cannot bear external manipulation. The mode of employing external massage is almost exclusively by friction, a process which is easier to show than to describe. For gynæcological purposes it may be conducted as follows: The palm of the hand is laid flat, and supported upon the venter of the ilium, the rami of the pubis, the surface of the sacrum, or the psoas muscles; then, while the abdominal muscles are kept as relaxed as possible, with the tips of two or more fingers the operator seeks from the front or from the side to work down upon the part or organ to be influenced, and to draw it against one of the underlying parts, or to press it down upon or against some resisting object.

5. Internal massage divides itself into *active* and *passive*. The active form consists in a process of kneading, of pulling, and pushing, by means of the two hands working together, the fingers of one being placed in the vagina (more rarely in the rectum), of the other on the abdomen. The passive form depends on the introduction of cylinders into the vagina, for the purpose of widening and of stretching the tissues. Active, or "combined massage," is that which has hitherto been most frequently practiced. It consists in kneading the affected part—that is to say, the operator tries from the vagina and abdominal surface to take the part between the fingers, and to rub, and press, and squeeze it. With this process of kneading may be associated a form suitable for gynæcological cases, consisting in pressing and drawing the part in various directions, according to the object to be attained. It is specially efficacious in dealing with cicatricial bands, and often affords better results, with less pain, than the kneading process.

Few women can tolerate the process more than once or twice a day, for from ten to fifteen minutes each time. Apart from the pain, a marked impression is produced upon the nervous system; sexual excitement can easily be avoided, but there is a nervous excitement of quite a different character, which is often produced by gynæcological manipulations, even in the most unimpressionable women. Only the courses of treatment which are regularly and systematically carried out with as few interruptions as possible are followed by any satisfactory result; the performance of the process of active massage only twice or three times a week produces little or no effect; the only means by which active massage can be reduced to two or three sittings a week is by employing passive massage in the intervals. This is similar to that sometimes carried out in the preparation of a patient for the operation of vesico-vaginal fistula when there are cicatrices in the vagina, or in cases of vaginismus. A series of vulcanite cylinders are employed to gradually dilate the parts contracted by cicatrix or spasm.

6. Massage may be advantageously combined with baths or with other therapeutic agents, such as iodine or iodoform.

7. (*a*) An absolute contra-indication is every acute disease, even if it runs its course without fever, and every sub-acute disease which has been accompanied by rise of temperature in its acute stage. In cases of exudation it is a safeguard against mishaps if we make two to three months' absence of fever a condition of employing massage; in the case of hæmatocele we may make a beginning six to eight weeks from its occurrence.

(*b*) Conditional contra-indications are: pregnancy, suspicion of consumption, perhaps latent gonorrhœa. In the first month of pregnancy we may cautiously employ active massage upon painful cicatricial bands. In the later months passsive massage may be used for cicatrices of the vagina from whatever cause. In one case, from a consumptive family, with catarrh of the apices an attack of pleurisy seemed to have been set up by the process. In ten cases undoubted latent gonorrhœa was followed by very painful joint affections, which were associated with slight fever.

Of the results obtained, from July, 1882, to July, 1884, 102 women were treated by massage. In the beginning 16 would not go on with the treatment, from the pain produced and due to the inexperience of the operator. Of the remaining 86, five were cases of new formations—three of these were cases of small intra-ligamentary tumors, two disappeared without any concomitant bad symptoms and with no return. The attempt to aid the gradual enucleation

of submucous fibroids failed in two cases. Thirteen times was massage undertaken for prolapse of the uterus. Only once, in a case of slight prolapse, was a cure effected; twice there was slight improvement. In marked prolapse or in complete procidentia, there was never any kind of improvement. Ten cases of chronic metritis and subinvolution were treated by massage, they were all of specially long duration and obstinacy, so that a complete cure in four cases and improvement in three is a very noteworthy result. Of 18 cases of exudation (including 5 cases of hæmatocele) 8 were completely cured (including 2 of hæmatocele), 2 were relieved to the extent of an entire absence of symptoms, and a less degree of improvement occurred in three. No case was met with in which fresh inflammation was set up in an old exudation, but extreme caution was used in applying massage to exudation cases.

The largest class of cases suitable for massage are the old cicatricial, contracted remnants of exudation, callous cicatricial bands upon a basis of simple inflammation, with the changes in the position of the uterus and ovaries depending upon these conditions. Out of 40 such cases, 24 were cured, and of 10 improved, 8 were in a clinical sense also cured.

It would be foolish to call these results brilliant; it would, however, be a mistake to overlook their value. If we remember that in every case other therapeutic measures had preceded, and when we think how little success we have attained in the cure of old exudations and the dissipation of obstinate cicatricial bands, we must admit that the results here obtained are sufficient to challenge a further trial of massage in gynæcology.

Influence of Age on Primiparous Labor.—As in comparison with the views of Courtade on this subject, given on page 409 of this journal, we append the views of Kleinwächter (*Zeit. f. Geburt u. Gynäk.*, *Practitioner*) who found his material in the records of 920 cases of primiparæ in his clinic at Innsbruck. These cases he divides into three groups, namely: (I.) 16 to 19 years of age, 111 cases; (II.) 20 to 29 years of age, 694 cases; (III.) 30 to 41 years of age, 115 cases. These groups he designated as the young, the middle-aged, and the old, respectively. From his study of these cases he draws the following conclusions: (1.) Accidental complications, which have nothing to do with pregnancy, occur least often in the youngest primiparæ and most frequently in the old. (2.) Ailments attributable to pregnancy are observed most frequently in the old and next most frequently in the young. (3.) Hæmorrhages in the course of pregnancy occur most frequently in the young and least frequently in the old. (4.) The duration of labor is most frequently abnormally protracted in the old; in this respect the young stand next to the old. (5.) Inefficient pains, on account of which the duration of labor is abnormally protracted, are least often observed in primiparæ in the bloom of their sexual life, *i. e.*, from 20 to 29, and most frequently in the old. (6.) Therefore forceps must be used most frequently in the old and least often in the middle-aged. (7.) The lengthening of the labor of primiparæ with the increase of age occurs chiefly in the first stage; the second stage is scarcely affected by differences of age; the third stage is not at all affected. (8.) The mortality per cent. after forceps operations on primiparæ rises parallel with the increase in age. (9.) The older the primipara, the greater is the danger of perineal laceration. (10.) The older the primipara, the more likely a post-partum hæmorrhage, although the frequency of hæmorrhage is by no means so great as hitherto supposed. (11.) With increase of age increases the disposition of primiparæ to affections of the kidneys. (12.) The frequency of œdema without kidney disease also increases with the age. (13.) The older the primipara, the less the danger of mastitis, and the less also the likelihood of her ability to suckle. (14.) The old most frequently, the middle-aged least frequently, sicken and die of puerperal fever; the same is true of puerperal mania. (15.) The morbidity and mortality per cent. is highest in the old and lowest in those from 20 to 29 years of age. (16.) Spontaneous premature labor occurs very frequently in old primiparæ, and least often in the middle-aged. (17.) With increase of age the frequency of abnormal positions of the fœtus increases. (18.) The older the primipara is, the more likely is she to bear a boy, except those from 20 to 21 years of age, who bear more girls than boys. (19.) Analogous to the discovery made by Hecker and confirmed by Wernich, that first-born children are heavier and longer the older the mothers are, is the fact that the umbilical cord of the first-born of old mothers falls off the earliest, and that of the first-born of the youngest mothers the latest. (20.) The liability to twin pregnancy in primiparæ increases with their age. (21.) With increase of age in primiparæ the frequency of bearing deformed children diminishes. (22.) The mortality per cent. of first-born children increases with the mothers' age; among the oldest primiparæ the fœtal mortality reaches a not inconsiderable height.

MEDICINE.

Experiments on Chronic Drunkenness.—During the last three years MM. Dujardin-Beaumetz and Andigé have worked at this subject with a perseverance which has justly gained the approval of men of science in all countries. The animals chosen for experimenting upon were pigs, for these reasons, that it is so easy to make them partake of alcoholic substances in their foods, and also that their digestive organs are very similar to those of human beings. Eighteen of these animals, aged 4 months, were submitted to the same *regime* at Grenille. They were given daily to each kilogramme of their food, weighed out as follows, 1–2 grammes of any one of the following spirits: Ethylic alcohol at 100°, potato spirit ten times rectified at 49°, potato spirit purified by charcoal at 46°, distillery grains at 92°, potatoes at 73°, grains at 37°, beetroot at 34°, spirit of beetroot rectified at 92°, and trade amylic acid at 90°. As long as the dose did not exceed one gramme

there were no difficulties of digestion, but when 1½ to 2 grammes were given the pigs lost their appetite, and their organs became impaired. Thus, it was necessary to suspend the treatment occasionally, to prevent serious results. The symptoms were found to be exactly similar to those of human beings suffering from chronic drunkenness. As regards the digestive functions, there were—loss of appetite, bilious or shiny vomiting, and bloody-mucus diarrhœa. There was also observed anomalous redness of stomachic mucus, occasionally congestion of the intestines, and once or twice violent ecchymoses comparable to the spots to be seen on the intestinal mucus of dogs dying of acute alcohol. These gastro-intestinal anatomical symptoms were much less marked than the functional troubles. The bilious secretion was also abnormal, as was proved by the yellow color of the conjunctivæ and urine, but in the autopsy the congestion of the liver and the friability of the parenchyma were only observed during the complete absence of interstitial hepatitis and acidity. The urine was little changed, only presenting a bilious appearance. The renal tissue was greasy, but this is very common among pigs. Nearly all the pigs suffered from a cough, and at the autopsy the lungs were shown to be congested, and sometimes in an apoplectic condition; the former was caused by the elimination of alcohol by the air tubules, and by the cold during the experiments. The organs of circulation were unaltered. The modifications of the nervous system were shown by drunkenness, the animals falling into a torpor soon after digesting the alcohol, but the ordinary excitement was not visible. After a time trembling was noticed, and want of strength in the limbs. The experiments, however, did not interfere with the fattening of the animals, but their sale for food was prohibited, as there existed in the flesh interstitial hæmorrhage of the muscular parenchyma.—*The Medical Press.*

THE DIFFERENTIAL DIAGNOSIS OF AMBLYOPIA.—M. Danir (*Bulletin de la Société Francaise d'Ophthalmologie; Med. Times*) finds that in the normal state, and in amblyopia without any lesion, a luminous impression is produced on the eye by the closure of an electric current of less intensity than one-tenth of a milli-ampère, whilst in cases where the optic nerve is undergoing atrophy, a current of much greater intensity is needed to produce the same minimum luminous impression. It was especially in respect of prognosis that he found this test the most useful. When, in a case of amblyopia, whether the disc was pale or not, the electrical reaction was normal, he found that the sight was eventually recovered; and, on the other hand, when the electrical reaction was diminished, then the prognosis as to recovery of sight was unfavorable. The *modus operandi* is simple. The positive pole is placed on the forehead, midway between the eyes, and maintained there by an india-rubber band; the negative pole, which is olive-shaped, is made to touch the upper and outer part of the eyeball. The circuit is then completed, and a sufficiently strong current passed to produce a very evident luminous impression, in order that there may be no mistake, and the patient may understand what it is that he is to be on the lookout for. When this has been explained to him, the strength of the current is very gradually diminished until only at the closure of the current there is perceived the very faintest spark, which is not recognized at the opening. In health it is, as has been already stated, only necessary to employ a current of the intensity of one-tenth of a milli-ampère to obtain this, and this serves as the standard whereby to measure various degrees of impairment. In optic atrophy, sclerosis of the optic nerves, atrophy after neuritis, after embolism or hæmorrhage, the electric spark is only produced by a stronger current. In amblyopia from alcohol or tobacco the electrical reaction is normal, as it also is in congenital amblyopia and in some cases of hysterical amblyopia.

SURGERY.

ECHINOCOCCUS OF THE NECK.—Dr. Lindpainter, of Munich, reports a case of the above in the late *Festschrift des Arztlichen Verein*, (*The Medical Press.*) The patient had noticed an enlargement of the neck, principally on the left side, from his earliest youth. About ten years ago, after forced yawning it increased rapidly in size, so that a considerable tumor developed behind the sterno-cleido-mastoid muscle, which caused difficulty of breathing, and was diagnosticated by various physicians as struma. On August 20, 1882, Dr. Lindpainter was called to perform the operation of tracheotomy on account of the difficulty in breathing. He found on the left side of the neck a tumor the size of a man's fist, reaching from the clavicle to the ear. The trachea was forced over to the right, and flexed, so that respiration was exceedingly difficult, and the œsophagus was also so much compressed that fluids even could only be swallowed with difficulty. Dr. Lindpainter took the tumor for a solid struma, calling for tracheotomy, but upon examining more closely he discovered in a small spot above the clavicle distinct fluctuation. After puncture and the evacuation of about three ccm. of clear watery fluid, fluctuation was noted in another part; he punctured this also, and let out about 5 ccm. of similar fluid, after which the trachea straightened itself, and the breathing at once became easy. Examination of the escaped fluid showed the presence of hooklets, and the only further question was the extirpation of the echinococcus. He determined upon incision and drainage. He accordingly incised and inserted a drain on September 2, and in the beginning of December, as no more cysts by that time appeared, the drainage-tube was removed, and in a day or two the wound healed, and at the date of the report the neck appeared quite well.

THE

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THE EDITOR of this JOURNAL would be glad to receive any items of general interest in regard to local events, or matters that it is desirable to call to the attention of the profession. Letters written for publication or containing items of information, should be accompanied by the writer's full name and address, although not necessarily to be published. All communications in regard to editorial work should be addressed to the Editor.

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SATURDAY, DECEMBER 20, 1884.

THE USEFULNESS OF THE NOSE IN DIAGNOSIS.—Probably every physician sees times in his examination of patients, when he devoutly wishes he had no olfactory nerves, or, since to be deprived of the sense of smell would at the same time also deprive him of the possibility of certain keen enjoyments, he prays for that happy condition of the farmer, who, when asked by a young theologian, if he objected to his saying grace at the table, replied, "Oh, no! say what you have a mind to, you can't turn my stomach." Unpleasant as the possession of a nose often is, there are occasions when its delicate appreciation of odors is fortunate. Nay, furthermore, it seems sometimes serviceable in guiding a physician to a correct diagnosis. All are familiar with the fetor of the air expired in cases of pulmonary gangrene. Likewise, the breath of patients suffering from chronic copper and lead poisoning has a foul, strongly suggestive smell.

A peculiar smell of the breath in other diseases has been repeatedly noted by trustworthy observers. Thus, a urinous odor has been detected in the breath of patients suffering from retention of urea in the system from renal disease. The act of coition is stated by a German whose name we cannot recall, to impart a very characteristic odor to the breath. It is needless to allude to disordered stomachs and constipated bowels, bronchorrhœa, decayed teeth, nasal catarrh, and numberless other conditions familiar to every physician, as productive of a foul breath that often leads to a diagnosis upon the very entrance of a patient into the consulting office.

The fetor of uterine cancer is so proverbial that its very mention seems superfluous. *Apropos* of this offensive smell, Dr. Middleton Goldsmith, of Rutland, Vt., in a contribution to the New York *Medical Record*, Nov. 29, 1884, entitled, "Some New Facts and Considerations in the Diagnosis of Cancer of the Stomach and Cancer of the Pancreas," cites a case in which the eructations emitted a fetor identical with that of carcinoma uteri. Gastric symptoms usually met with in cancer of the stomach were not marked, though a pronounced cachexia was present. The autopsy revealed an encephaloid cancer involving almost the entire stomach. Upon being accidentally torn into, the organ emitted an overpowering stench identical with that having distinguished the eructations in life. Dr. Goldsmith says this fetor is not invariably present, but that, when it obtains, it may be regarded as pathognomonic of gastric cancer. The fact is well worth remembering.

Peculiar odors do not proceed exclusively from mucous membranes, the skin sometimes emits an unnatural smell in disease.

A pungent mice-like odor is said to be characteristic of typhus.

A sour smell is sometimes perceived to proceed from persons ill with acute articular rheumatism. We do not recall any reference in medical literature, to a singular odor of the perspiration sometimes encountered in phthisical patients. But we recollect vividly two instances in which we were struck by this peculiarity. The first case was that of a man who had a very extensive consolidation of both apices.

The odor did not impress us specially, until a few minutes afterward upon approaching another patient, already stripped for examination, when the same odor met us, surrounding the man like a cloud. At once, without considering the rashness of the statement, we exclaimed to those about, this is a case of chronic consolidation of the lungs. Luckily the examination sustained the diagnosis. In these instances, the odor could not have been due to the medicines taken, as these were devoid of special scent. Whether there be anything in this or not, peculiar odors associated with some diseases are so well established as to render the nose of the diagnostician anything but superfluous.

SECTION of Practice of Medicine and Materia Medica of the American Medical Association:

The Chairman of this Section earnestly requests all who intend to present papers in it during the coming meeting in New Orleans, to forward the titles and

table of contents of such papers to Dr. H. D. Didama, Chairman, Syracuse, N. Y., or to Dr. Garland, Secretary, Hotel Aubery, Boston, Mass., as early as possible, that they may aid the Committee of Arrangements in having the full programme printed and ready for the use of members who may attend the meeting in New Orleans.

The same thing is desirable in reference to all who expect to read or present papers in any of the Sections of the Association.

The Chairman or Secretary of the Section to which the paper would belong should be notified of the title and contents of the paper as early as possible; and the officers of all the Sections should report to the Chairman of the Committee of Arrangements at least one month before the time of the annual meeting. We urge attention to this now, because heretofore the Chairman of the Committee of Arrangements has been embarrassed and delayed every year, in making up and printing the programme of work for the annual meeting, on account of the neglect of writers to give proper and timely notice of their intention to present papers.

CHOLERA BACILLI.—The statements in regard to the effects of this micro-organism are as variable or contradictory as ever.

While Dr. E. Van Ermengen in his communication to the Microscopical Society of Belgium claims to have successfully inoculated dogs, rabbits and guinea-pigs with the cultivated comma-bacilli, Dr. Klein has swallowed liberal doses of bacilli in pure culture, and Dr. Bochefontaine, chief of Vulpian's laboratory, has himself taken a drachm of the intestinal discharge from a cholera patient in the form of pills, both without any effect.

LIST OF PERMANENT MEMBERS OF THE AMERICAN MEDICAL ASSOCIATION.—A by-law or standing rule of the Association requires that a full list of the Permanent Members shall be published in the Transactions every third year. In complying with this rule it will be necessary to occupy the next number of the JOURNAL, which will be the closing one of the third volume, almost wholly with the said list of members, the Librarian's Annual Catalogue of additions to the Library, and the title page with full index of the volume.

COCAINE HYDROCHLORATE.—Under the head of Foreign Correspondence will be found an interesting letter on this and other preparations derived from the coca leaves.

THE CHOLERA appears to have entirely disappeared from France with the exception of Toulon, and the disease lingers in only a few places in Italy.

A NATIONAL BOARD OF HEALTH.—Our readers will remember that a National Conference of State Boards of Health was held in St. Louis during the recent meeting of the American Public Health Association in that city, in which measures were taken to influence the action of Congress in favor of the reëstablishment of a National Board of Health. The members of the Conference appointed to further this object were to reassemble in Washington soon after the opening of the present session of Congress. We are informed that there were present at this last Conference delegates from twenty-five States, and forty principal cities of the United States and Canada. The result has been the preparation of the following bill and its presentation to the Committee on Public Health, by whom it is expected the same will be brought before the two houses of Congress. The bill is as follows:

A BILL.

To amend an Act entitled "An act to prevent the introduction of contagious and infectious diseases into the United States and to establish a National Board of Health."

Be it enacted by the Senate and House of Representatives of the United States of America in Congres assembled, That an Act entitled, "An act to prevent the introduction of contagious and infectious diseases into the United States and to establish a National Board of Health," approved March 3, 1879, be so amended as to provide that there shall be established a National Board of Health, to consist of one member from each State Board of Health now established, or which may be hereafter established in the United States, to be appointed by the President and confirmed by the Senate, whose compensation when actually engaged in the performance of duty under this act, shall be ten dollars per diem each, and reasonable expenses. This board shall meet in Washington within ninety days after the passage of this Act, and shall meet in Washington annually, and in case of emergency upon the call of its chairman and secretary, or upon the extraordinary call of the President of the United States, as hereinafter provided.

The officers of this board shall be a chairman and secretary. The Secretary shall be the executive officer of and ex officio a member of the board, and shall devote his entire time to the duties of the office, and may be removed for cause, at any regular meeting of the board, two-thirds of the full board voting therefor, and shall receive such salary as may be determined by the board. The chairman with six other members, representing the various geographical divisions of the country, shall constitute the executive committee of the board, to be elected at the first meeting of the board, and at each annual meeting thereafter, and said committee shall, and is hereby authorized to exercise such powers as may from time to time be conferred upon it by the board.

SEC. 2. The duties of this board shall be, and it is hereby authorized and given power to make or cause

to be made, such investigations at any place within the United States, or at any foreign port or place, and to collect information upon all matters relating to the public health, and to frame such rules and regulations as may be necessary for the government of the quarantine service of the United States; and all the power and authority now provided by law, or which may be provided by law, for the control and protection of the public health of the United States, shall be and are hereby vested in said board, except as to the special authority vested in the President of the United States under the provisions of this act. The rules and regulations of this board shall severally be executed, under the direction of this board, through such Departments of the Government, or other officers, as the law may prescribe or the President may designate.

This board shall coöperate with, and so far as it lawfully may, shall aid State and local boards of health in the enforcement of the rules and regulations of such boards, to prevent the introduction of contagious and infectious diseases from foreign countries into the United States, and into one State from another.

SEC. 3. It shall be the duty of this board to make such rules and regulations as are necessary to be observed by vessels at ports of departure, where such vessels sail from any foreign port or place to any port or place in the United States to secure the best sanitary condition of such vessel, her cargo, passengers and crew, and to prepare from time to time for the consular officers of the United States, and for the medical officers serving under this act at any foreign port, and otherwise make publicly known such rules and regulations, which, when approved by the President and issued by the Department of State, and posted in the office of the consul or other representatives of the United States at such foreign ports for at least ten days, shall be enforced by the consular officers and agents of the United States.

SEC. 4. It shall be unlawful for any vessel from any foreign port or place to enter any port in the United States, except in accordance with the rules and regulations made in pursuance of this act, and of the rules and regulations made under State or municipal authority, and any such vessel which shall attempt to enter any port of the United States in violation thereof, shall be liable to process in the proper district court of the United States, and upon conviction, shall forfeit to the United States a sum to be awarded in the discretion of the court, not exceeding $1,000, which shall be a lien upon such vessel to be recovered upon proceeding in the proper district court of the United States, in accordance with the rules and laws governing cases of seizure of vessels for violation of the revenue laws, and in all such cases the United States attorney for such district shall appear on behalf of the United States; and all such vessels shall obtain from the consular or authorized medical officer at the port of departure, a certificate in duplicate, setting forth the sanitary history of said vessel, and that it has in all respects complied with the rules and regulations of this board, made in pursuance of this act for the government of such vessels, and before granting such certificate such consular or medical officer is required to be satisfied that the statements therein made are true; and upon the request of this board, the President of the United States is hereby authorized to appoint proper medical officers, to serve in the offices of the consuls at any such foreign ports, to make the inspections and give the certificate herein required.

SEC. 5. Such vessels shall observe all rules and regulations made by this board in pursuance of this act, in regard to the inspection, disinfection and isolation of the same, upon its arrival at any port in the United States and for the treatment of persons and cargo on board, so as to prevent the introduction of contagious diseases into the United States, and it shall be unlawful for any vessel to enter such port, to land its passengers or discharge its cargo except upon a certificate from the health officer of such port, that such rules and regulations have in all respects been complied with.

SEC. 6. In the event of any sudden emergency, threatening the importation of contagious or infectious disease into the United States from any foreign country, the President of the United States is hereby authorized and required, in his discretion, to adopt and make known forthwith by public proclamation, such measures as may meet the emergency, either by suspending the introduction into the United States by land or sea, of any specified merchandise calculated to be a vehicle for the communication of contagion, or by prohibiting the entry into the ports of the United States of vessels coming from infected countries or having contagious or infectious disease on board. And in case the President shall at any time exercise the authority hereby conferred upon him, he shall, at or before the time of issuing such proclamation as aforesaid, convene the National Board of Health, to meet at Washington in special session within ten days from the date of such notice of convention, and the said board shall thereupon advise such measures as it may deem sufficient to meet the emergency; and upon the taking effect of such measures, with the approval of the President of the United States as herein provided, the President's proclamation aforesaid shall cease to have effect. It shall be the duty of this Board at all times to give prompt attention to any question in sanitary science which may be submitted to it by the President.

SEC. 7. It shall be the duty of the Department of State, to obtain from the consular officers at foreign ports or places, all available information in regard to the sanitary condition of such ports and places, and to transmit the same to this board; and it shall be the duty of this board to obtain from the State and municipal health authorities throughout the United States, and from all other available sources, weekly reports of the sanitary condition of ports and places within the United States, and reports and other matters relating to climatic and other conditions affecting the public health, and it shall prepare, publish and transmit to State and other authorities, and other proper persons, weekly abstracts of such reports, consular reports, and other useful information relating to the public health; and it shall make to the President,

for transmission to Congress, an annual report of its transactions, with such recommendations as it may deem important to the public health; and the necessary printing of the board shall be done at the Government Printing Office, upon the requisition of the secretary of such board, in the same manner and subject to the same provisions as other public printing for the several Departments of the Government.

SEC. 8. The President of the United States is authorized, when requested by this board, and when the same can be done without prejudice to the public service, to detail officers from the several Departments of the Government, for temporary duty, to act under the direction of this board in carrying out the provisions of this act, and such officers shall receive no additional compensation, except for actual and necessary expenses incurred in the performance of such duties.

SEC. 9. To meet the expenses incurred in carrying out the provisions of this act, the sum of $500,000, or so much thereof as may be necessary, is hereby appropriated, to be disbursed under the direction of the board; and the board shall have authority to appoint such disbursing agents as it deems necessary, who shall give bond as in other cases, for the faithful performance of their duties.

SEC. 10. All acts and parts of acts in conflict with any of the provisions of this act shall be, and are hereby, repealed.

Assuming that there should be some responsible and efficient national health organization, as an aid to the Government in preventing the introduction and spread of infectious and contagious diseases, it becomes a very important question whether such organization should consist of a National Board composed of one member from each State in the Union, as proposed in the above bill, or of a regular Government Bureau of Health, with a single responsible medical man at its head, on the same basis as the Bureau of Education, or of Agriculture, as was advocated by Dr. D. J. Roberts in his address as Chairman of the Section of Hygiene and State Medicine of the American Medical Association at its meeting in May last. We trust the whole subject will receive a full and impartial consideration both by Congress and the medical profession.

SOCIETY PROCEEDINGS.

CHICAGO MEDICAL SOCIETY.

At the semi-monthly meeting held on the evening of December 1, 1884, the regular order of scientific and other important business was upon motion, temporarily suspended, and the following appropriate remarks and resolutions of respect relative to the death of Dr. Abram Groesbeck, an old and well-known physician and Fellow of the Society whose demise occurred November 25, 1884, at the age of 74 years, 7 months and 15 days, were introduced by Dr. R. C. Hamill and unanimously adopted.

LISTON H. MONTGOMERY, M.D.,
Secretary.

MR. PRESIDENT AND FELLOW MEMBERS OF THE CHICAGO MEDICAL SOCIETY:—I arise to offer a tribute of respect to the memory of a beloved member of this Society, who in the Providence of God, has been suddenly summoned from our midst to appear at the council chamber of the Great Physician.

Death has made many reprisals from the ranks of our profession in these last years. Some who had just entered the portals of professional life, with bright prospects of eminent success in full view, have been cut off in the very flower of their days—others in the vigor and maturity of their years manfully bearing their part in the battle of life have fallen in full clad armor in the front ranks—but he to whom I call your attention to-night, had more than filled his cycle of three score years and ten, and a half century of that time has been devoted to professional services at the bedside of the sick and suffering.

"The leaves of the oak and willow shall fade,
Be scattered around and together be laid;
And the young and the old, and the low and the high,
Shall moulder to dust and together shall lie."

* * * * *

"The hand of the King that the sceptre hath borne,
The brow of the Priest that the mitre had worn;
The eye of the Sage and the heart of the brave,
Are hidden and lost in the depth of the grave."

Dr. Abram Groesbeck was no common man, but a learned and scientific Doctor of Medicine, and a man of decided character, as Fellows present, who have had a more intimate association with him than has been my fortune, in the remarks that are to follow, I am sure will take great pleasure in declaring.

He lived a life of usefulness, and the world is better for such living—he died in the fullness of his years, his intellect unclouded and his love for the literature of the profession as ardent as in the vigor of his days, an honor to the cause to which the energies of a well spent life had been devoted, respected by his fellow practitioners, revered and affectionately beloved by the families who for so many years have enjoyed the benefits of his professional skill.

WHEREAS, This Association is called to mourn the loss of one of its beloved members, who, for more than a quarter of a century, has lived the life of an honorable and successful physician in our city;

Resolved, That we contemplate with great pleasure the well-rounded character of Dr. Abram Groesbeck, who has so faithfully filled the measure of a true American physician, and commend his example as a Christian and a physician to those of our number who are entering upon the arduous and responsible duties of our profession. We regret that we are called upon to part with him forever—that we shall meet him no more at the bedside of suffering—in the social circle or in the busy walks of daily life.

Resolved, That whilst we regret to note upon the records of our Society the demise of our greatly respected fellow, yet it is with pleasure we contemplate the fact, that up to the final summons, his men-

tal faculties remained in full strength, and that he was able to contemplate with perfect calmness the approaching catastrophe, saying in that prompt and decisive manner so characteristic of him: "I have accomplished my mission here, and it is better to go now, than suffer the pains and aches incident to extreme age."

Resolved, That we admire the learned and gentlemanly physician, and cherish the example he left us as a rich inheritance, and claim the privilege of dropping a tear of sincere sympathy with the bereaved family, in this, the day of their sorrow.

Resolved, That a copy of these resolutions be sent to the family of the deceased by the Secretary of this Society.

Dr. John Bartlett spoke as follows:

Mr. President: I desire to say a few words in memoriam of the friend and fellow from whom death has just separated us.

Dr. Abram Groesbeck was born at Albany, N. Y., May 24, 1810. He began his medical studies in his native city, becoming a Licentiate of the New York State Medical Society in 1831, and graduating at Albany in 1849. He settled in Chicago in 1856, and continued to practice medicine actively till a few years since, retiring only when he was no longer able to see to write a prescription. It was a circumstance which rendered the very last days of his life happier that "loosed from his environment" in part, as he was by impairment of sight and hearing, and other infirmities, he yet had a few patients to visit. For some years past symptoms of angina pectoris had developed, and two months since he had a violent attack, from which, however, he recovered, and he was in his usual health till the 24th ult., when he was taken without pain with Cheyne-Stoke's respiration, the period of apnœa alternating with rapid breathing each minute. Meanwhile the action of the heart grew feebler for eighteen hours, when unconsciousness supervened, soon followed by death. Dr. Groesbeck was fully aware of his condition, and yet he spoke cheerily to all, not omitting jocosely to rally the profession upon its want of understanding of, and lack of cure for, his ailment. The doctor expressed himself as prepared and willing to depart this life, declaring that his work was done.

He was a member of the Episcopal Church.

Dr. Groesbeck had a tall and graceful figure and a fine face. His features strikingly resembled those of the late Dr. Gross. In manner he was a gentleman of the old school—genial, courteous, amiable. He was of a kindly spirit and benevolent disposition, his most prominent characteristic being sacrifice of self for the advantage of others.

Dr. Groesbeck was endowed with a superior intellect. He received a classical education which led to an appreciation of good literature and the cultivation of excellent taste for the best writers, ancient and modern. His familiarity with the classics he retained in a remarkable degree. It was one of his amusements to confound a friend by leaving his compliments and message on his slate in the language of Celsus. He retained also his knowledge of the French. Even in the last months of his life he selected for reading such writings as those of Tacitus; and only recently he planned to have read to him again the Æneid of Virgil. It gave him great pleasure to exalt the matter and style of the ancients, and to credit them with knowledge, philosophy and rhetorical skill. He liked to dwell among the old moderns, and read with deep interest such works as Pepy's Diary. On one occasion he insisted on leading a medical friend into the Public Library, that he might there read how the physician of one of his favorite old diarists successfully prescribed, hundreds of years ago, milk diet as a cure for diseases of the kidney. Of late years the doctor had been more interested in religious works, as in "Sceptical Fallacies," by W. J. Hall; and in the volume of Henry Drummond, "Natural Law in the Spiritual World." The latter production he pronounced remarkable.

"One of the saddest reflections of my later years," said recently one of the ablest of Chicago's medical men, "is that the many minor means and arts which experience has given me for the cure of disease and the relief of suffering will perish with me." Dr. Groesbeck was an old and skillful practitioner, and yet, so far as I am aware, he has left no record of his experience. It occurs to me as proper here to note a few of the views, which memory now recalls, held by him as the sum of his experience in certain departments of practice. However opposed the views of the doctor may prove to those commonly promulgated by the professional teacher, it is well to grant due weight to the opinions of so experienced and sagacious a practitioner. The doctor was by nature sceptical, and he was ever on his guard lest he might lend credence to some view not thoroughly established. While interested in theory, he was, preferring methods of true empiricism, loth to acknowledge its value. To the enthusiastic germ theorist who might occupy his attention with ingenious possibilities as to the cause of disease, he was ever ready with his chilling query, "*cui bono?*" Long before the German cry "*Fort mit der spray*," was heard over the land, Dr. Groesbeck, when invited to witness an ovariotomy, satirically inquired, "Are they to *play* with the spray?" Dr. Groesbeck often declared himself a disciple of Dr. Todd of London, and for years past had treated pneumonia not as a phlegmasia, but as a form of fever; in some cases giving a bottle of wine in twenty-four hours with apparent advantage. Even the statistical information, furnished since the death of Dr. Todd, adverse to his ideas as to the value of the stimulant treatment, did not change the opinion or practice of Dr. Groesbeck.

Dr. Groesbeck had ever before him in the practice of his profession the strong influence of the mind over the body of the sick. He deemed it a part of the skill of the physician to protect his patient from anxieties and fear. His manner and converse in the sick-room, therefore, were such as to inspire hope and courage in those under his care. It may be stated that Dr. Groesbeck deemed it in practice the part of wisdom to avoid suggesting a consultation; and this, on the ground that it diminished the confidence of the patient in his attendant, and with confidence

shaken, no small part of the power of treatment for good he believed to be lost.

For a number of years past the doctor was afflicted with a degree of deafness precluding anything like satisfactory auscultation. It was remarkable how little this circumstance lessened his confidence in diagnosing diseases of the chest. He trusted entirely to acute observation, and shrewd interpretation of the ordinary symptoms.

The doctor looked upon the placenta blocking the os uteri or vagina after labor as a natural and most useful tampon, and he regarded too early an interference with it as a mischievous interruption of nature in her efforts to close and occlude the uterine vessels. Naturally he agreed with Braithwaite, that in certain conditions of hæmorrhage after labor, the uterus being contracted, to tampon was good practice; and he went so far as to suggest that probably in such cases the re-introduced placenta might be the best plug that the accoucheur could employ. The doctor had for nearly forty years of his practice an unusual number of cases of post-partum hæmorrhage. He had meantime conducted the third stage of labor according to the standard precepts of the day. Of late years he had fewer cases of hæmorrhage, and he referred the change in his experience to a change in his practice. As an almost invariable rule he had during the first decades of his work given ergot immediately after the birth of the child. It was following a discontinuance of this practice that the comparative freedom from post-partum bleeding occurred.

In placenta prævia centralis Dr. Groesbeck believed that perforation of placenta gave as good results as the passing of the hand between it and the uterus, and he regarded the former method as easier, and more speedy than the latter. When under any circumstances turning had been resorted to, Dr. Groesbeck delivered promptly, impressing on the child the necessary turns, and never waiting for nature to do so. In breech presentations the doctor thought it better practice to break the wedge as soon as the os was sufficiently dilated, bringing down both feet and delivering at once.

The doctor was an advocate of the rather frequent use of the forceps, using the old model Bedford for higher operations. When the head was lower in the pelvis he used the short forceps, always speaking highly of their convenience and efficiency. It should be remarked, however, that in the selection of the short instrument, Dr. Groesbeck rejected those in the construction of which the maker, in shortening the handles, shortened also the cephalic curve.

Dr. Groesbeck fully endorsed the views of the anatomy of the cervix uteri gravidi held by Braune, Bandl, Barnes and others. He believed that the placenta prævia was located below the os internum.

The doctor was possessed of an unusual degree of candor and was entirely void of professional pretense. He often admitted his shortcomings; thus, he did not hesitate to state that the mechanism of labor as regards the mutations of the fœtal head in passing the parturient canal were to him inexplicable. He declared that he was generally unable to determine the position of the head, and that the reputed "planes and spines" effecting changes in it were to him a "terra incognita."

His favorite obstetrical work was "The Principles of Midwifery, by John Burns." In the preference here indicated our colleague complimented his own judgment as well as the classical production of the Glasgow professor.

The doctor had a keen appreciation of the ways and means which tact and experience teach as aids to success in practice. In a knowledge of the subtle methods by which a physician may ingratiate himself with the people, magnify his importance, and obtain and retain practice, Dr. Groesbeck was not second to the author of "The Physician Himself." But while he was prodigal of advice in such matters to others, he took, in his own conduct, little heed of his own wisdom.

After he had been in the profession half a century he felt less confident that he would in any case do all that was possible for a patient than he had felt fifty years before. He on one occasion expressed surprise at the recollection of the nonchalant unconcern with which as a novice he had undertaken the management of an adherent placenta or placenta prævia.

As to the profession as a business, Dr. Groesbeck held an opinion contrary to that of the great mass of practitioners. He always insisted that the practice of medicine was upon the whole as easy and as pleasant a mode of earning a livelihood as any of which he had knowledge.

Dr. Groesbeck felt a fatherly interest in the younger members of the profession, and never tired of stimulating their thought and enlarging their knowledge by the narration of instructive cases and the enunciation of cardinal precepts.

Mr. President: From the shores of the mystical river we call to our vanishing friend the final word of mortals: Vale! Vale! An adieu laden with uncertainty, yet buoyant with hope. As we turn from the dark waters to the duties yet before us, let us pause to inscribe our index memorabilium with the honorable name of Abram Groesbeck.

FOREIGN CORRESPONDENCE.

COCAINE.

WIESBADEN, GERMANY, Dec. 1, 1884.

Mr. Editor:

E. Merck, the celebrated pharmaceutist of Darmstadt, has kindly furnished me with the following items in regard to the now famous remedy—cocaine.

The chemical formula according to Lossen is: $C_{17} H_{24} NO_4$. Cocaine is the alkaloid contained in the coca leaves (Erythoxylon Coca Lam) which was first isolated by Niemann. In 1862, Lossen discovered a second principle contained in the same leaves, viz.: hygein, which is of a volatile nature, and has received but little investigation so far; it seems to have a weak and hardly characteristic action. The leaves further contain ecgonin, coca tannin and a peculiar wax.

The coca crystals belong to the monocline system, melt at 98° C., easily soluble in alcohol, more easily

in ether, but only dissolve in 704 parts water. The cocaine *salts*, however, are easily soluble in water.

The first reports of the internal use of coca leaves are handed down from the sixteenth century (Dr. Monardes, Seville, 1569). Its first appearance in Europe dates from 1749—it was described by Jussieu, and was named erythoxylon coca by Lamarck. Tschudi, Markham, Poppig and others who travelled in South America, found that the natives were in the habit of chewing coca leaves as a remedy for or preventive against the effects of extraordinary exertions. It seems, however, that the Indians chewed the leaves, together with the ashes of chenopodium. Quinea, the alkali in these ashes, probably eliminated the tannin from the coca leaves, leaving free the alkaloid.

Cocaine is believed to be the really active principle of the coca leaves. At first, on apparently good grounds, it was believed that this alkaloid possessed a property analogous to caffeine, theine and theobromine, viz.: the checking of waste in the body, but Merck declares it impossible to absolutely confirm this theory. Only the other day I saw it in the same list with guarana, caffeine, theine, etc., but was not considered to be as powerful as caffeine!

Cocaine has in small doses an exhilarating effect on the nerve centres, and it takes but a very small dose to produce toxic effects, especially in cold-blooded animals, but fortunately the effects of the drug do not seem to be cumulative. Schroff, who first experimented with cocaine in 1862, observed that 0.05 grm. given internally to rabbits caused great variation in the pulse and respiration, also temporary mydriasis; the same dose, subcutaneously, caused death in convulsions of an epileptic form, also mydriasis in a strong degree, which disappeared at once after death. In frogs 0.001 grm. produced complete loss of motion, preceded by excitement, while a dose of 0.002 grm. proved fatal.

According to Fronmüller, in 1863, a dose of 0.03 –0.33 grm. given internally did not have any important effect upon human beings, simply causing sleep in some, accelerated and then retarded pulse and respiration in others. In a case of attempted suicide 1.5 grams produced no serious injury. Explanatory of this Mr. Merck suggests that the so-called cocaine of that time may not have been really pure cocaine. The preparation with which experiments have been and are being made on this side is Merck's cocaine murias sol. and the dose is 0.05 grm.; however, as much as 0.5 grm. of this solution has been administered subcutaneously. The effects of a subcutaneous injection are first a feeling of warmth, then an insensibility to feeling in the neighborhood of the part, and finally a reddening of the skin. In thirty minutes' time these all disappear.

Dr. Aschenbrandt has found in cocaine an admirable restorative from the exhaustion of diarrhœa, as stated in No. 50 of the *Deutsche Medicin. Wochenschr.*, 1883. Prof. Dr. E. V. Fleischel, of Vienna, has proved, to his own satisfaction at least, that cocaine subcutaneously administered is a valuable aid in preventing and curing the morphia habit. (I myself have had excellent results from it with tobacco chewers who were anxious to break the habit.) In America, W. H. Bently observed the good effects of cocaine in overcoming the taste for morphia. Dr. Freud, of the Vienna General Hospital, reports a case of the morphia habit cured within ten days by the use of 0.1 grm. injected subcutaneously three times daily. His theory is that morphia and cocaine are antagonistic. Dr. Freud likewise credits this alkaloid with aphrodisiac properties.

According to the experiments collected by Merck, the therapeutical value of cocaine may be thus described: Cocaine is a stimulant which quickly increases and serves to sustain, in a harmless manner, the physical powers of the body; it is much more strengthening than alcohol—hence its value on all occasions of extraordinary bodily exertion—in doses of 0.05 grm. at intervals. Whether the powers of the mind can likewise be increased and strengthened is still unknown, as also whether the activity of the nerve centres can be permanently increased. Some good results have been obtained in the treatment of melancholia by the subcutaneous method. It is valuable as a stomachic, particularly after a debauch in eating or drinking. It is credited with the permanent cure of atonic indigestion. The various cachexias, phthisis, exhausting fevers, mercurial poisoning, etc., are said to be averted by its use. Its value in dipsomania is not so well established as in the morphia habit, though there are many satisfactory experiments in that direction. As is well known, cocaine paralyzes the sensation of the mucous membrane when brought in contact with the membrane; hence its possible future in diseases of the throat, and in operations about the mouth and throat. (I have not seen any account of experiments of others, but I am making some observations and trials in regard to the use of the drug subcutaneously in perineorrhaphy and trachelorrhaphy. It will be a happy day for surgeons if these minor operations can be done without ether.)

Of its ophthalmological value enough has already been said.

Cocaine citras is used by dentists to anæsthetize the dental nerves. Mr. Merck tells me that this salt can be readily kneaded into pills, which may be wrapped in wadding, moistened and placed in the hollow of the tooth, when lo! the tooth can then be cleaned and filled without pain. If this be true, the man who discovered it is worthy of a high place among the benefactors of the race.

Merck's preparations include the pure cocaine alkaloid, also the salts, viz.: Hydrochlorate, salicylate, hydrobromate, tartrate and citrate. The experiments mentioned above have been made with the hydrochlorate in about 0.05 gramme doses. Von Hoffman, of Baden-Baden, recommends the salicylate for ophthalmological use, but Merck observes that it still remains to be seen which salt is the best for the eye. Finally, cocaine is not a simple substance. When treated with concentrated hydrochloric acid, it splits up into ecgonin, benzoic acid, and methyl alcohol. Mr. Merck informs me that he hopes to be able ere long to report on the physiological action of ecgonin.

That your readers may possibly be as much interested in this subject as I am, is my only apology for thus intruding upon your valuable space.

Very sincerely yours,

SARAH HACKETT STEVENSON, M.D.

BOOK REVIEWS.

COURIER REVIEW CALL BOOK. J. H. Chambers & Co., Chicago, St. Louis and Atlanta.

PHYSICIANS' POCKET DAY BOOK. By C. H. LEONARD, Detroit.

THE 1885 MEDICAL RECORD VISITING LIST. Wm. Wood & Co., N. Y.

THE PHYSICIAN'S VISITING LIST. P. Blakiston, Son & Co., Philadelphia.

The character of these books is too well known to need comment. They are all excellent for the purpose for which they were intended. They are well bound, and in every way convenient.

MISCELLANEOUS.

COMFORTS FOR TRAVELLING INVALIDS.

An article on Winter Health Resorts, in the *Practitioner*, contains some valuable hints to invalids, to relieve the discomforts found by them abroad in travelling and at hotels. One of the discomforts of the long railway journey which is required in order to reach most health resorts, is the constant vibration and the fatigue it occasions. This may be, to a considerable extent, avoided by the proper use of air cushions. It is well for an invalid to be provided with two or more of these. The one to be used for sitting on may be either round or square, and may be either with or without a depression in the centre. Air cushions of a horseshoe shape and furnished at the ends with tapes so that they can be opened out, and again tied together, are most useful. One of them tied round the loins supports the back, and another just like a collar round the neck supports the head, so that in whatever position it is put, sideways, backwards, or forwards, it always rests against the cushion.

Another comfort to invalids travelling, is an India rubber bag for hot water with a flannel case. Indeed it is well to have two of these if the invalid be very delicate. If any sudden chill is felt, or if any pain in the chest or elsewhere should come on, relief is often obtained by the application of a hot-water bottle which takes the place of a poultice. The invalid's friend can easily get these bottles filled with hot water by giving a small gratuity to the guard of the train, and asking him to fill them when stopping at a station. Stoppages occur on the French lines at least every two hours, and the water retains its heat quite long enough to make it useful between the stoppages. Another thing which is a great comfort is an eider-down coverlet, which should be both long and broad. It is useful, not only during travelling by land or by sea, but during a stay at the health resorts. One would imagine that an eider-down quilt would be bulky and inconvenient to travel with, but this is not the case. In most "hold-alls" there is a large pocket, and the eider-down quilt can be readily rolled up and put into this pocket. When the straps are once around it, it can, from its softness, be easily pressed into a small bulk.

Two other things that the invalid should not be without are a small etna, with can or kettle, to boil water, and a small package of tea, also beef-tea in a portable form, with a small box of biscuits. Even when such things are to be had at a hotel, the prices put upon them are sometimes so exorbitant that the invalid would hardly care to order them, however much he might desire them.

OFFICIAL LIST OF CHANGES IN THE MEDICAL CORPS OF THE U. S. NAVY DURING THE WEEK ENDING DECEMBER 13, 1884.

Craig, T. C., Passed Assistant Surgeon. Detached from Alliance and placed on waiting orders. Dec. 6, 1884.

Curtis, L. W., Assistant Surgeon. To the Naval Hospital, Chelsea. Dec. 9, 1884.

Gaines, J. H., Passed Assistant Surgeon. Present duty continued.

Green, E. H., Passed Assistant Surgeon. Special duty at Museum of Hygiene, Washington. Dec. 11, 1884.

Murray, J. M., Passed Assistant Surgeon. Detached from Naval Hospital, Chelsea, to the Flagship Hartford. Dec. 9, 1884.

Wolverton, T., Surgeon. To the Shenandoah. Dec. 6, 1884.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, UNITED STATES ARMY, FROM DECEMBER 6, 1884, TO DECEMBER 12, 1884.

Hammond, John F., Colonel and Surgeon, retired from active service, by operation of law, on Dec. 7, 1884, under provision of Act of Congress approved June 30, 1882. (S. O. 287, A. G. O., Dec. 8, 1884.)

Irwin, B. J. D., Major and Surgeon, granted one month's leave of absence. (S. O. 112, Dept. Arizona, Nov. 28, 1884.)

McKee, J. C., Major and Surgeon. Leave of absence still further extended one month. (S. O. 288, A. G. O., December 9, 1884.)

O'Reilly, Robt. M., Captain and Assistant Surgeon, assigned to duty as attending Surgeon, Washington City, D. C., to date from October 20, 1884. (S. O. 284, A. G. O., December 4, 1884.)

Porter, Joseph Y., Captain and Assistant Surgeon, sick leave of absence extended four months on surgeon's certificate of disability. (S. O. 286, A. G. O., Dec. 6, 1884.)

Kane, John J., Captain and Assistant Surgeon, from Dept. East to Willets Point, New York. (S. O. 286, A. G. O., December 6, 1884.)

Banister, J. M., Captain and Assistant Surgeon. (Ft. Adams, R. I.) Granted one month's leave of absence on surgeon's certificate of disability. (S. O. 251, Dept. East, December 9, 1884.)

Barrows, C. C., First Lieutenant and Assistant Surgeon, in addition to other duties, to take charge of Medical Division Office, Dept. Arizona, during absence of Surgeon B. J. D. Irwin. (S. O. 112, Dept. Arizona, Nov. 28, 1884.)

Gray, Chas. C., Major (retired.) Died at Geneva, N. Y., Nov. 22, 1884, instead of Nov. 26, as heretofore announced. (Circular Orders A. G. O. December 8, 1884.)